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*Cover Illustration.* A court in session, as depicted in a German law book of the early sixteenth century, *Der neu Layenspiegel von rechtmässigen ordnungen in Burgerlichen und peinlichen Regimenten* (Augsburg, 1511), fol. cliiʳ. (See William J. Bouwsma, "Lawyers in Early Modern Culture," pp. 303–27.)

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# Contents

VOLUME 78 • NUMBER 2 • APRIL 1973

## Articles

Lawyers in Early Modern Culture, BY WILLIAM J. BOUWSMA 303

Politics and Patricide in Freud's *Interpretation of Dreams*, BY CARL E. SCHORSKE 328

Tempest in Clio's Teapot: The American Historical Association Rebellion of 1915, BY RAY ALLEN BILLINGTON 348

T.S. Kuhn's Theory of Science and Its Implications for History, BY DAVID A. HOLLINGER 370

## Review Article

Hitler and Psychohistory, BY HANS W. GATZKE 394

WALTER C. LANGER. The Mind of Adolf Hitler: The Secret Wartime Report

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# Reviews of Books


## GENERAL

J. H. HEXTER. *The History Primer;* J. H. HEXTER. *Doing History.* By W. Warren Wagar 402

MELVIN SMALL, ed. *Public Opinion and Historians: Interdisciplinary Perspectives.* By Thomas J. Pressly 403

NATHAN G. HALE, JR., ed. *James Jackson Putnam and Psychoanalysis: Letters between Putnam and Sigmund Freud, Ernest Jones, William James, Sandor Ferenczi, and Morton Prince, 1877–1917.* By Mary R. Dearing 404

MAX SCHUR. *Freud: Living and Dying.* By Richard L. Schoenwald 405

LEON Ó BROIN. *Fenian Fever: An Anglo-American Dilemma.* By Galen Broeker 406

EDWARD H. SIMS. *Fighter Tactics and Strategy, 1914–1970;* RICHARD C. LUKAS. *Eagles East: The Army Air Forces and the Soviet Union, 1941–1945.* By James J. Hudson 407

CLARENCE G. LASBY. *Project Paperclip: German Scientists and the Cold War.* By A. Hunter Dupree 408

## ANCIENT

ERNST POSNER. *Archives in the Ancient World.* By George W. Houston 408

E. A. LOWE. *Palaeographical Papers, 1907–1965.* 2 vols. Ed. by LUDWIG BIELER. By Revilo P. Oliver 409

J. W. WEVERS and D. B. REDFORD, eds. *Studies on the Ancient Palestinian World, Presented to Professor F. V. Winnett on the Occasion of His Retirement, 1 July 1971.* By Morton Smith 410

BENJAMIN MAZAR, ed. *The World History of the Jewish People.* 1st ser., *Ancient Times (Patriarchs and Judges),* vols. 2, 3. By Cyrus H. Gordon 411

F. E. WINTER. *Greek Fortifications.* By Michael H. Jameson 413

## MEDIEVAL

JOSEPH R. STRAYER and DONALD E. QUELLER, eds. *Post Scripta: Essays on Medieval Law and the Emergence of the European State, in Honor of Gaines Post.* By Brian Tierney 413

J. P. TRABUT-CUSSAC. *L'administration anglaise en Gascogne sous Henry III et Édouard I de 1254 à 1307.* By B. Wilkinson 415

MAURICE BERESFORD and JOHN G. HURST, eds. *Deserted Medieval Villages: Studies.* By Colin Platt 416

W. O. AULT. *Open-Field Farming in Medieval England: A Study of Village By-Laws.* By P. D. A. Harvey 417

C. R. CHENEY. *Notaries Public in England in the Thirteenth and Fourteenth Centuries.* By Elisabeth G. Kimball 418

JOHN HATCHER. *Rural Economy and Society in the Duchy of Cornwall, 1300–1500.* By Sylvia L. Thrupp 418

V. J. SCATTERGOOD. *Politics and Poetry in the Fifteenth Century, 1399–1485.* By J. R. Lander 419

EDWARD GRANT, ed. and tr. *Nicole Oresme and the Kinematics of Circular Motion:* Tractatus de commensurabilitate vel incommensurabilitate motuum celi. By Claudia Kren 420

KLAUS ARNOLD. *Johannes Trithemius (1462–1516).* By Gerald Strauss 421

## MODERN EUROPE

HENRY RAYNOR. *A Social History of Music: From the Middle Ages to Beethoven.* By Walter L. Woodfill 422

MICHALINA VAUGHAN and MARGARET SCOTFORD ARCHER. *Social Conflict and Educational Change in England and France, 1789–1848.* By Evalyn A. Clark 422

GUILLAUME DE BERTIER DE SAUVIGNY. *Metternich et la France après le Congrès de Vienne.* Vol. 3, *Au temps de Charles X—1824/1830.* By Paul W. Schroeder 423

RAYMOND J. SONTAG. *A Broken World, 1919–1939.* By Istvan Deak 424

ALLEN F. CHEW. *The White Death: The Epic of the Soviet-Finnish Winter War;* THEDE PALM. *The Finnish-Soviet Armistice Negotiations of 1944.* By John H. Hodgson 425

W. A. PANTIN. *Oxford Life in Oxford Archives.* By Sheldon Rothblatt 426

L. A. CLARKSON. *The Pre-Industrial Economy in England, 1500–1750.* By H. S. Reinmuth, Jr. 427

G. R. BATHO. *A Calendar of the Shrewsbury and Talbot Papers in Lambeth Palace Library and the College of Arms,* vol. 2. By Willson H. Coates 428

LEONA ROSTENBERG. *The Minority Press & the English Crown: A Study in Repression, 1558–1625.* By Robert Munter 428

R. C. RICHARDSON. *Puritanism in North-west England: A Regional Study of the Diocese of Chester to 1642;* ERIC C. WALKER. *William Dell: Master Puritan.* By Paul S. Seaver 429

JOHN X. EVANS, ed. *The Works of Sir Roger Williams.* By Paul Stewart 430

JAMES M. OSBORN. *Young Philip Sidney, 1572–1577.* By Robert W. Kenny 431

LELAND H. CARLSON, ed. *The Writings of John Greenwood and Henry Barrow, 1591–1593.* By W. B. Patterson 431

W. J. JONES. *Politics and the Bench: The Judges and the Origins of the English Civil War.* By Stuart E. Prall 432

*The Diary of Thomas Isham of Lamport (1658–*


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*81), Kept by Him in Latin from 1671 to 1673 at His Father's Command.* Tr. by NORMAN MARLOW. By Charles F. Mullett 433

J. R. WESTERN. *Monarchy and Revolution: The English State in the 1680s.* By Robert Walcott 433

*Calendar of State Papers Preserved in the Public Record Office.* Domestic Series. *James II.* Vol. 3, *June, 1687–February, 1689.* By Stephen B. Baxter 434

RICHARD S. TOMPSON. *Classics or Charity? The Dilemma of the 18th Century Grammar School.* By Donald H. Akenson 435

GORDON JACKSON. *Hull in the Eighteenth Century: A Study in Economic and Social History;* R. G. WILSON. *Gentlemen Merchants: The Merchant Community in Leeds, 1700–1830.* By W. E. Minchinton 435

J. T. WARD and R. G. WILSON, eds. *Land and Industry: The Landed Estate and the Industrial Revolution. A Symposium.* By John Norris 436

JAMES A. HENRETTA. *"Salutary Neglect": Colonial Administration under the Duke of Newcastle.* By Philip S. Haffenden 437

OWEN CHADWICK. *The Victorian Church.* Pt. 1, *1829–1859;* pt. 2, *1860–1901.* By Josef L. Altholz 438

GLEN PETRIE. *A Singular Iniquity: The Campaigns of Josephine Butler.* By Walter L. Arnstein 440

J. B. CONACHER. *The Peelites and the Party System, 1846–52;* WILBUR DEVEREUX JONES and ARVEL B. ERICKSON. *The Peelites, 1846–1857.* By William H. Maehl, Jr. 440

PAUL SMITH, ed. *Lord Salisbury on Politics: A Selection from His Articles in the* Quarterly Review, *1860–1883;* ANDREW JONES. *The Politics of Reform, 1884.* By Peter Marsh 441

BRIAN L. BLAKELEY. *The Colonial Office, 1868–1892;* R. J. HIND. *Henry Labouchere and the Empire, 1880–1905.* By Joseph O. Baylen 443

A. B. COOKE and J. R. VINCENT, eds. *Lord Carlingford's Journal: Reflections of a Cabinet Minister, 1885.* By Trevor Lloyd 444

W. J. READER. *The Weir Group: A Centenary History.* By Gordon Rimmer 444

RODNEY BARKER. *Education and Politics, 1900–1951: A Study of the Labour Party.* By Catherine Ann Cline 445

C. J. LOWE and M. L. DOCKRILL. *The Mirage of Power.* Vol. 1, *British Foreign Policy, 1902–14;* vol. 2, *British Foreign Policy, 1914–22;* vol. 3, *The Documents: British Foreign Policy, 1902–22.* By Arthur Marsden 445

IAN H. NISH. *Alliance in Decline: A Study in Anglo-Japanese Relations, 1908–23;* WM. ROGER LOUIS. *British Strategy in the Far East, 1919–1939.* By M. A. Fitzsimons 446

LAWRENCE F. BARMANN. *Baron Friedrich von Hügel and the Modernist Crisis in England.* By William J. Schoenl 447

PETER ROWLAND. *The Last Liberal Governments: Unfinished Business, 1911–1914.* By Henry R. Winkler 448

J. M. WINTER and D. M. JOSLIN, eds. *R. H. Tawney's Commonplace Book.* By Bernard Semmel 449

KENNETH YOUNG. *Sir Alec Douglas-Home.* By Stephen R. Graubard 449

EDWARD NORMAN. *A History of Modern Ireland;* PATRICK O'FARRELL. *Ireland's English Question: Anglo-Irish Relations, 1534–1970.* By Joseph Woods 450

DONALD HARMAN AKENSON. *The Church of Ireland: Ecclesiastical Reform and Revolution, 1800–1885.* By Olive J. Brose 452

RICHARD COBB. *Reactions to the French Revolution.* By R. R. Palmer 452

ROGER DUPUY. *La Garde Nationale et les débuts de la révolution en Ille-et-Vilaine (1789–mars 1793).* By Charles Tilly 453

FRANCISCO MARTÍ GILABERT. *El motín de Aranjuez;* ANGEL MARTÍNEZ DE VELASCO. *La Formación de la Junta Central.* By Richard Herr 454

MICHAEL GLOVER. *Legacy of Glory: The Bonaparte Kingdom of Spain, 1808–1813.* By Owen Connelly 455

RICARDO DE LA CIERVA. *Historia de la guerra civil española.* Vol. 1, *Perspectivas y antecedentes, 1898–1936.* By Stanley G. Payne 455

ARXIU VIDAL I BARRAQUER. *Església i estat durant la segona República espanyola, 1931–1936.* Vol. 1, *14 d'abril–30 d'octubre de 1931,* 2 pts. Ed. by M. BATLLORI and V. M. ARBELOA. By Joan Connelly Ullman 457

ERICH THOMSEN. *Deutsche Besatzungspolitik in Dänemark 1940–1945;* ALAN S. MILWARD. *The Fascist Economy in Norway.* By Heinz E. Ellersieck 457

CLAUDE NORDMANN. *Grandeur et liberté de la Suède (1660–1792).* By Walther Kirchner 458

REINHARD HÖHN. *Scharnhorsts Vermächtnis,* 2d ed. By Peter Paret 459

THEODORE S. HAMEROW. *The Social Foundations of German Unification, 1858–1871: Struggles and Accomplishments.* By James Joll 460

ANDREW R. CARLSON. *Anarchism in Germany.* Vol. 1, *The Early Movement.* By Carl Landauer 461

*Akten zur deutschen auswärtigen Politik, 1918–1945, aus dem Archiv des Auswärtigen Amts.* Ser. C: *1933–1937. Das Dritte Reich: Die ersten Jahre,* vol. 1, pts. 1, 2. By Gerhard L. Weinberg 462

MIRON CONSTANTINESCU *et al. Unification of the Romanian National State: The Union of Transylvania with Old Romania.* Ed. by MIRON CONSTANTINESCU and ŞTEFAN PASCU. By Stephen Fischer-Galaţi 463

DOUGLAS DAKIN. *The Unification of Greece, 1770–1923.* By L. S. Stavrianos 464

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JOSEPH T. FUHRMANN *et al. Essays on Russian Intellectual History*. Ed. by LEON BORDEN BLAIR.
By Marc Raeff 464

S. FREDERICK STARR. *Decentralization and Self-Government in Russia, 1830–1870*.
By Alfred J. Rieber 465

RAYMOND T. MCNALLY. *Chaadayev and His Friends: An Intellectual History of Peter Chaadayev and His Russian Contemporaries*.
By Nicholas V. Riasanovsky 466

ANDREW D. KALMYKOW. *Memoirs of a Russian Diplomat: Outposts of the Empire, 1893–1917*. Ed. by ALEXANDRA KALMYKOW.
By Donald W. Treadgold 467

ANATOLE G. MAZOUR. *The Writing of History in the Soviet Union*. By Warren B. Walsh 467

ABRAHAM ROTHBERG. *The Heirs of Stalin: Dissidence and the Soviet Regime, 1953–1970*.
By Robert V. Daniels 468

AFRICA

JOHN S. MBITI. *New Testament Eschatology in an African Background: A Study of the Encounter between New Testament Theology and African Traditional Concepts*. By G. Wesley Johnson 469

UNITED STATES

THOMAS C. COCHRAN. *Business in American Life: A History*. By William N. Parker 469

IVOR NOËL HUME. *A Guide to Artifacts of Colonial America*. By Lois Green Carr 471

WESLEY FRANK CRAVEN. *White, Red, and Black: The Seventeenth-Century Virginian*.
By Peter H. Wood 471

JOHN W. REPS. *Tidewater Towns: City Planning in Colonial Virginia and Maryland*.
By Cary Carson 472

DAVID H. FLAHERTY. *Privacy in Colonial New England*. By Daniel Scott Smith 473

WILLIAM G. MCLOUGHLIN. *New England Dissent, 1630–1833: The Baptists and the Separation of Church and State*. 2 vols. By John B. Boles 474

JAMES T. LEMON. *The Best Poor Man's Country: A Geographical Study of Early Southeastern Pennsylvania*. By John M. Murrin 475

JAMES H. HUTSON. *Pennsylvania Politics, 1746–1770: The Movement for Royal Government and Its Consequences*. By Gary B. Nash 476

S. K. STEVENS *et al.*, eds. *The Papers of Henry Bouquet*. Vol. 1, *December 11, 1755–May 31, 1758*.
By John Shy 477

MARSHALL SMELSER. *The Winning of Independence*; DON HIGGINBOTHAM. *The War of American Independence: Military Attitudes, Policies, and Practice, 1763–1789*; IRA D. GRUBER. *The Howe Brothers and the American Revolution*.
By William B. Willcox 478

BARBARA GRAYMONT. *The Iroquois in the American Revolution*. By James L. Axtell 480

EDWIN A. BEILHARZ. *Felipe de Neve: First Governor of California*. By Warren A. Beck 481

VAN BECK HALL. *Politics without Parties: Massachusetts, 1780–1791*. By Richard D. Brown 482

DAVID F. LONG. *Nothing Too Daring: A Biography of Commodore David Porter, 1780–1843*.
By Robert Erwin Johnson 482

JOSEPH M. HAWES. *Children in Urban Society: Juvenile Delinquency in Nineteenth-Century America*; RAYMOND A. MOHL. *Poverty in New York, 1783–1825*. By Michael Frisch 483

DWIGHT F. HENDERSON. *Courts for a New Nation*.
By Robert G. Seddig 484

*Journal of the Senate of Virginia. November Session, 1793*; JOHN A. WILLIAMS, ed. *Journals and Proceedings of the General Assembly of the State of Vermont, 1791–1792*.
By R. Nicholas Olsberg 485

DONALD B. COLE. *Jacksonian Democracy in New Hampshire, 1800–1851*. By Paul H. Bergeron 486

C. VANN WOODWARD. *American Counterpoint: Slavery and Racism in the North-South Dialogue*.
By Winthrop D. Jordan 486

GERALD SORIN. *Abolitionism: A New Perspective*; GERALD SORIN. *The New York Abolitionists: A Case Study of Political Radicalism*; *The Letters of William Lloyd Garrison*: vol. 1, *I Will Be Heard! 1822–1835*, ed. by WALTER M. MERRILL; vol. 2, *A House Dividing against Itself, 1836–1840*, ed. by LOUIS RUCHAMES; WILLIAM B. HIXSON, JR. *Moorfield Storey and the Abolitionist Tradition*.
By Bertram Wyatt-Brown 488

DAVID M. POTTER. *The South and the Concurrent Majority*, ed. by DON E. FEHRENBACHER and CARL N. DEGLER; WILLIAM C. HAVARD, ed. *The Changing Politics of the South*. By George B. Tindall 490

JOSEPH S. O'FLAHERTY. *An End and a Beginning: The South Coast and Los Angeles, 1850–1887*.
By John E. Baur 492

JOHN F. REIGER, ed. *The Passing of the Great West: Selected Papers of George Bird Grinnell*.
By Larry D. Ball 492

DAMON WELLS. *Stephen Douglas: The Last Years, 1857–1861*. By Thomas B. Alexander 493

GEORGE H. MILLER. *Railroads and the Granger Laws*.
By Robert L. Peterson 494

DAVID M. EMMONS. *Garden in the Grasslands: Boomer Literature of the Central Great Plains*.
By Gilbert Stelter 494

TAMARA K. HAREVEN, ed. *Anonymous Americans: Explorations in Nineteenth-Century Social History*.
By John Modell 495

ANDREW M. GREELEY. *That Most Distressful Nation: The Taming of the American Irish*.
By Robert Dougherty Cross 496

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HARALD S. NAESS and SIGMUND SKARD, eds. *Studies in Scandinavian-American Interrelations, Dedicated to Einar Haugen*. By Carlton C. Qualey 497

LARS LJUNGMARK. *For Sale—Minnesota: Organized Promotion of Scandinavian Immigration 1866–1873*; EMORY LINDQUIST. *Vision for a Valley: Olof Olsson and the Early History of Lindsborg*. By Franklin D. Scott 497

RUDOLF GLANZ. *Jew and Italian: Historic Group Relations and the New Immigration (1881–1924)*. By Luciano J. Iorizzo 498

GEORGE J. PRPIC. *The Croatian Immigrants in America*. By Dimitri Kitsikis 499

JOHN TOMSICH. *A Genteel Endeavor: American Culture and Politics in the Gilded Age*. By Robert C. Bannister 500

O. GENE CLANTON. *Kansas Populism: Ideas and Men*; BURTON J. WILLIAMS. *Senator John James Ingalls: Kansas' Iridescent Republican*; WILLIAM WARREN ROGERS. *The One-Gallused Rebellion: Agrarianism in Alabama, 1865–1896*. By Robert F. Durden 501

WILLIAM M. DICK. *Labor and Socialism in America: The Gompers Era*. By Staughton Lynd 502

WAYNE FLYNT. *Duncan Upshaw Fletcher: Dixie's Reluctant Progressive*; FRED GREENBAUM. *Fighting Progressive: A Biography of Edward P. Costigan*; RICHARD LOWITT. *George W. Norris: The Persistence of a Progressive, 1913–1933*; LEROY ASHBY. *The Spearless Leader: Senator Borah and the Progressive Movement in the 1920's*. By Philip De Vencentes 502

HAZEL W. HERTZBERG. *The Search for an American Indian Identity: Modern Pan-Indian Movements*. By Nancy Oestreich Lurie 505

HENRY CLEPPER. *Professional Forestry in the United States*. By Harold K. Steen 506

CHARLES CHATFIELD. *For Peace and Justice: Pacifism in America, 1914–1941*. By David A. Shannon 506

GEORGE T. BLAKEY. *Historians on the Homefront: American Propagandists for the Great War*; WILLIAM SUMMERSCALES. *Affirmation and Dissent: Columbia's Response to the Crisis of World War I*. By Kevin J. O'Keefe 507

RONALD C. TOBEY. *The American Ideology of National Science, 1919–1930*. By Hamilton Cravens 508

WALTER SMITH SCHOENBERGER. *Decision of Destiny*; AVERILL A. LIEBOW. *Encounter with Disaster: A Medical Diary of Hiroshima, 1945*. By Alvin D. Coox 509

GEORGE F. KENNAN. *Memoirs*. Vol. 2, *1950–1963*. By Richard W. Leopold 510

LATIN AMERICA

A. OWEN ALDRIDGE, ed. *The Ibero-American Enlightenment*. By George M. Addy 511

SHERBURNE F. COOK and WOODROW BORAH. *Essays in Population History: Mexico and the Caribbean*, vol. 1. By Donald B. Cooper 512

JOHN RUTHERFORD. *Mexican Society during the Revolution: A Literary Approach*. By Charles A. Hale 513

HÉLIO SILVA. *1938: Terrorismo em Campo Verde*. By John W. F. Dulles 514

MARVIN GOLDWERT. *Democracy, Militarism, and Nationalism in Argentina, 1930–1966: An Interpretation*. By Robert A. Potash 514

Communications 516

Recent Deaths 519

Association Notes 521

Other Books Received 522

Index of Advertisers 77(a)

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# Lawyers and Early Modern Culture

WILLIAM J. BOUWSMA

ALTHOUGH EUROPEAN HISTORIANS have increasingly recognized the impact of large-scale change or significant events on human culture, they have paid little attention to the importance of the less dramatic aspects of social experience for shaping the attitudes of men. The result has been, for most of us, a schism between social and intellectual history that has impoverished both. As Frederic C. Lane has reminded us, the routine tasks of daily life are likely to impress those engaged in them with a profound sense of what the world and especially men are like and to produce patterns of expectation and systems of value—dimensions of culture in its larger meaning.[1] Eventually these impressions are likely to find explicit formulation in philosophy, science, theology, and literature and the other arts—in culture in a narrower sense. But since the work by which men support their needs tends, particularly in the modern world, to be highly differentiated, it is difficult to treat the relation of work to culture in general terms. To get at this relationship, the historian must examine the experience of particular occupational groups that have held a position of strategic importance both in movements central to their social universe and in the articulation of its vision of the human condition. The rise and development of groups of this kind, especially where they have not previously been prominent, gives the historian an opportunity, unique in its concreteness, to study the sources and the nature of social change. In addition such groups may be especially useful for identifying the sources in social experience of fundamental shifts in attitudes and values. Scientists and technologists invite this kind of study in our own time. So, for early modern Europe, do lawyers.

In view of the attention recently directed to social history, it is remark-

This article, presented in an earlier form at the annual luncheon meeting of the Modern European History Section of the American Historical Association, New York, December 29, 1971, owes much to the critical reading of my colleagues Gerard E. Caspary, Thomas G. Barnes, and John T. Noonan, Jr.; the latter two are themselves lawyers as well as historians. I am particularly grateful to Mr. Thomas H. Reynolds, associate director of the Law School Library at Berkeley, for help in illustrating this article.

[1] Frederic C. Lane, *Venice and History* (Baltimore, 1966), 427–28.



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able that so little study of occupational groups has been done. One reason, I take it, is the tendency of many social historians to rely for most purposes on the categories of social class, which, though only imperfectly related to occupational differences, can usually be made to absorb them. From this standpoint occupations are interesting chiefly because they help in the assignment of individuals to their appropriate classes, and small attention is given to the often rather different functions of men who are seen as members of the same class, or to the possibility of contrasting perceptions of life arising out of the quite different ways men spend their working days. Lawyers can doubtless be generally identified with the middle-class or some rank above it, though precision on this matter has often proved difficult: in societies sensitive to social gradation, men of the law regularly presented problems, a fact that might well alert us to their special significance. But the mind of a lawyer was also shaped by a professional experience that made him rather different from most merchants or landed gentlemen.[2]

A second obstacle to the fruitful study of occupations has been our tendency to leave them to specialists concerned primarily with disciplines abstracted from their human and social meaning. Thus we have histories of theology but few of theologians, with the result that theology presents itself chiefly as an evolving set of disembodied ideas rather than as a response to human needs perceived through some kind of experience with life. We have histories of science but few of scientists considered as a group, and histories of law—indeed great classics on this formidable subject—but very little on lawyers as a profession characterized by a certain social role and a particular perspective on life and the world. What we have is useful and often admirable; we need not go all the way with Christopher Hill's somewhat imperial assertion that "it is time to take legal history out of the hands of the lawyers, as religious history has been taken away from the theologians, and to relate both to social development."[3] Yet the general point is valid enough, even though—as revealed by recent works of Gilmore and Martines for Italy, of Franklin, Kelley, and Huppert for France, and, among others, of Thorne, Pocock, and Little for England—a broader interest in lawyers and their activity is now stirring.[4]

[2] See George Rudé, *Hanoverian London, 1714–1808* (Berkeley, 1971), 37. Rudé finds the occupational surveys of England by Gregory King (1696) and Patrick Colquhoun (1805) less instructive than Defoe's classification of Englishmen according to wealth (1709); but as a result lawyers, as a distinct group, largely disappear from Rudé's picture of London society.

[3] Christopher Hill, *Puritanism and Revolution: The English Revolution of the Seventeenth Century* (New York, 1964), 28. See also the stimulating remarks of Barbara J. Shapiro, "Law and Science in Seventeenth Century England," *Stanford Law Review*, 21 (1969): 728.

[4] Myron P. Gilmore, *Humanists and Jurists: Six Studies in the Renaissance* (Cambridge, Mass., 1963); Lauro Martines, *Lawyers and Statecraft in Renaissance Florence* (Princeton, 1968); Julian Franklin, *Jean Bodin and the Sixteenth Century Revolution in the Methodology of Law and History* (New York, 1963); Donald R. Kelley, *Foundations of Modern Historical Scholarship: Language, Law, and History in the French Renaissance* (New York, 1970); George Huppert, *The Idea of Perfect History: Historical Erudition and Historical Philosophy in Renais-*

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This article is concerned to call attention to some of the larger possibilities in the subject and thus to the need for further work that, with proper respect for differences among societies and legal traditions, will permit a higher level of generalization.

The history of lawyers is peculiarly adapted to display connections among various dimensions of human activity, but it also bears on fundamental controversies about the nature of the law itself. In the long history of speculation on the law, lawyers of the Renaissance were the first theorists since the Sophists to argue that legal systems should not be required to reflect the will of God in any direct way or to mediate between eternal reason and the practical needs of men;[5] the law functions best, some Renaissance lawyers maintained, as a response to the concrete needs of particular societies and in fact can be shown to have evolved in this way.[6] Ever since the Renaissance, men have divided over this issue or, like Sir Edward Coke, uneasily straddled it; and the same disagreement, or at any rate the profound psychological differences it may be taken to represent, underlies much contemporary dispute about the law, perhaps including recent debate over strict construction.[7] A more comprehensive study of the historical relations between law and social experience might also contribute something to this perennial issue; our subject may thus have some practical importance.

THE LEGAL PROFESSION of early modern Europe was a somewhat diverse body, and I shall define it rather loosely to include all those who supplied legal or quasi-legal services. At its highest and most broadly influential level of eminence it included judges and magistrates, sometimes with little or no formal education, who applied the law to the various needs of government. In addition it included juridical scholars with varying degrees of practical experience, practicing lawyers—among them canonists as well as civilians and common or customary lawyers—and also, in this period, notaries. Sometimes organized with lawyers in a single guild as in Renaissance Florence, notaries supplied various legal services to a far larger body of clients than their less numerous and more prestigious colleagues. Because the task of providing written legal instruments merged into the

*sance France* (Urbana, 1970); Samuel Thorne, "Tudor Social Transformation and Legal Change," *New York University Law Review*, 26 (1951): 10-23, and his *Sir Edward Coke, 1552–1952* (London, 1952); J. G. A. Pocock, *The Ancient Constitution and the Feudal Law: English Historical Thought in the Seventeenth Century* (Cambridge, 1957); David Little, *Religion, Order, and Law: A Study in Pre-Revolutionary England* (New York, 1969). These examples are limited to the area of my own concern, but it is encouraging to note that an interest in lawyers is also developing elsewhere, notably among American historians.

[5] See the admirable survey of early Western legal theory in Julius Stone, *Human Law and Human Justice* (Stanford, 1965), 9–31.

[6] See Kelley, *Foundations of Modern Historical Scholarship*, 289, *passim*.

[7] See Little's general treatment of Coke in *Religion, Order, and Law*, 167–217; see also Shapiro, "Law and Science," 727.

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*Fig. 1.* Spanish notaries of the late fifteenth century, as depicted in a woodcut in Francisco Aznar y García, *Indumentaria española* (Madrid [1881]), estampo 15.

work of public secretary and scribe, notaries were important in relating law to literature and scholarship. They were therefore of special importance in mediating between the broader and the narrower meanings of culture.[8]

It is, at any rate, evident that men somehow connected with the law occupy a remarkable and disproportionate place among those prominent in European cultural expression.[9] The connection was sometimes a matter of

[8] Some indication of the importance of notaries in Italy may be gleaned from *Il notariato nella civiltà italiana: Biografie notarili dall' VIII al XX secolo* (Milan, 1961). For the importance of notaries in the Italian communes, see Daniel Waley, *The Italian City-Republics* (London, 1969), 29. J. K. Hyde, *Padua in the Age of Dante: A Social History of an Italian State* (Manchester, 1966), 154–75, has much on notaries in Padua. For notaries in Florence, see Martines, *Lawyers and Statecraft, passim*; on their functions, see page 37. I have not seen Giorgio Costamagna, *Il notaio a Genova tra prestigio e potere* (Rome, 1970). It may be noted that in England scriveners also provided some legal services.

[9] The only systematic study on this point that I have seen is the appendix in Huppert, *Idea of Perfect History,* 185–93. Huppert finds that, of the writers listed in the *Bibliothèque françoise* of La Croix du Maine who "made significant contributions to French culture between 1540 and 1584" and about whom information was available, eighty per cent were connected with the law.

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degree, and in particular cases it was clearly ambiguous. Many young men destined for fame in other connections studied law but failed or refused to complete the course or in the end made no professional use of their legal training. Among these were Petrarch, Luther, both Calvin and his victim Servetus, the satirist Samuel Butler (so widely read in his own time), Voltaire, Diderot, and David Hume, who eventually became librarian of the Faculty of Advocates in Edinburgh. Although students of Calvin have never doubted the influence of the law on his formation, others mentioned above reacted violently against the law, though this, too, may testify to its importance. Such names also leave us with an impression of the extent to which ambitious fathers regarded a career in the law as an avenue of worldly success for gifted sons who might have had other views for themselves.[10] In still other cases the influence of the law was rather a matter of family background, with its subtle impressions, and of milieu, than of direct professional engagement. Machiavelli, for example, was the son of a lawyer who—we may surmise—might, had he been more prosperous, have sent the boy to study law, then an expensive affair. At any rate young Niccolò found a type of employment in the Florentine government usually reserved for lawyers or notaries, and he was deeply concerned throughout his life with the social function of law. The dramatist Beaumont was son of a justice of common pleas. Pascal and Racine were both sons of lawyers and magistrates and spent much of their lives close to the legal circles of Paris.[11]

But in many cases the relation to law was close and direct. Guicciardini had a flourishing legal practice in Florence for many years. La Boétie and Montaigne, before he retired to devote himself to letters, were judges in the Parlement of Bordeaux. John Donne attended Lincoln's Inn and served as a law clerk before taking orders. Corneille was an *avocat* and magistrate, like his father before him, and Molière studied and may even have practiced law before turning to the stage. La Fontaine was a magistrate. Leibnitz finished law school to become a professor of law at the age

[10] At the same time it may be observed that the uses of the law for social mobility have been somewhat exaggerated. Great legal careers tended to run in families in Italy and France, and in Britain the law was largely a monopoly of the gentry. On this point, for Florence (probably typical of Italy), see Martines, *Lawyers and Statecraft*, 68. In France, of course, membership in the high magistracy was largely hereditary; see J. H. Shennan, *The Parlement of Paris* (Ithaca, 1968), 110–12, and see also Marcel Rousselet, *Histoire de la magistrature française* (Paris, 1957), 1: 259–322. The family of Montaigne was typical; see Donald M. Frame, *Montaigne: A Biography* (New York, 1965), 7–8. For indications that a similar situation prevailed in Portugal, and probably Spain, see Stuart B. Schwartz, "Magistracy and Society in Colonial Brazil," *Hispanic American Historical Review*, 50 (1970): 721. For England, see E. W. Ives, "The Common Lawyers in Pre-Reformation England," *Transactions of the Royal Historical Society*, ser. 5, vol. 18 (1968): 160, and Lawrence Stone, "The Educational Revolution in England, 1560–1640," *Past and Present*, no. 28 (1964): 58; for Scotland, see John Clive, "The Social Background of the Scottish Renaissance," in N. T. Phillipson and Rosalind Mitchison, eds., *Scotland and the Age of Improvement* (Edinburgh, 1970), 228.

[11] Roberto Ridolfi, *Vita di Niccolò Machiavelli* (rev. ed.; Florence, 1969), 1: 25–35. On the costs of a legal education, see Martines, *Lawyers and Statecraft*, 70. Although I have reservations about Lucien Goldmann's general thesis in other respects, his interpretation of Pascal and Racine in the context of this group, in *Le dieu caché* (Paris, 1955), seems to me appropriate.

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of twenty-three. Giambattista Vico, though he never achieved the professorship to which he aspired, saw himself primarily as a student of civil law. The career of Montesquieu followed the pattern of Montaigne. Henry Fielding was a lawyer and magistrate in London.[12] And other great figures combined government service as lawyers with historical composition or religious and philosophical reflection: Thomas More, Paolo Sarpi, and Francis Bacon. Such an enumeration is, to be sure, impressionistic; but it could be carried much farther, and it has a certain cumulative impact, which is strengthened when we look at movements rather than men.

Thus the culture of Renaissance humanism, especially in its earlier stages, was largely a creation of lawyers and notaries.[13] Salutati was a notary, Bruni went from the study of law into the Florentine chancery, and Poggio and Flavio Biondo were notaries, as was Lorenzo Valla, the most original mind among the humanists of Italy. The son of a lawyer at the Curia, Valla felt enough confidence in his own understanding of the law to challenge the methods of its contemporary practitioners.[14] And the relations between humanism and the law were even closer in the French Renaissance. The great scholars and historians of sixteenth-century France were jurists, from Budé to Pasquier, Bodin, and Jacques de Thou. Most of the *libertins* of the earlier seventeenth century were lawyers. Lawyers played a large part in public support for the scientific movement of the seventeenth century in both France and England—the membership lists of the Royal Society, for example, are filled with their names—and the so-called Scottish Renaissance of the eighteenth century was dominated by lawyers.[15]

This is to say nothing of those great figures who devoted themselves primarily to juridical thought: Coke, Selden, Grotius, Pufendorf, Beccaria, and Bentham. The point is important, for it should be recognized that jurisprudence was not, in these centuries, only an esoteric and highly specialized discipline. Some knowledge of the law was essential equipment for the landed gentry of Britain in an age of lively litigation over

[12] Martines reviews Guicciardini's legal career in *Lawyers and Statecraft*, 110–12; on Montaigne's legal career, see Frame, *Montaigne*, 46–62. For Vico, see Dario Faucci, "Vico and Grotius: Jurisconsults of Mankind," in Giorgio Tagliacozzo and Hayden V. White, eds., *Giambattista Vico: An International Symposium* (Baltimore, 1969), 61. William Empson, "Tom Jones," *Kenyon Review*, 20 (1958): 217–49, discusses Fielding's fiction in the light of his experience with the law; this article was called to my attention by Paul Alpers.

[13] This is especially well brought out by Jerrold E. Seigel, *Rhetoric and Philosophy in Renaissance Humanism* (Princeton, 1968). See also Denys Hay, *The Italian Renaissance in Its Historical Background* (Cambridge, 1961), 69–72.

[14] Salutati's notarial career has received special attention in Peter Herde, "Politik und Rhetorik in Florenz am Vorabend der Renaissance," *Archiv für Kulturgeschichte*, 47 (1965): 155, and in Ronald G. Witt, "Coluccio Salutati, Chancellor and Citizen of Lucca," *Traditio*, 25 (1969): 191–216. For Valla, see Kelley, *Foundations of Modern Historical Scholarship*, 19–50.

[15] Kelley, *Foundations of Modern Historical Scholarship, passim;* Hans Baron, "Secularization of Wisdom and Political Humanism in the Renaissance," *Journal of the History of Ideas*, 21 (1960): 131–50; René Pintard, *Le libertinage érudit dans la première moitié du XVII^e siècle* (Paris, 1943); Shapiro, "Law and Science," 738; Clive, "Social Background of the Scottish Renaissance," 228–31.

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titles to real property and of the gentry's general responsibility, as justices of the peace, for the preservation of local order. In addition some acquaintance with the philosophical principles of the law was a part of the culture of every educated man. Laymen like Hobbes had an extensive knowledge of the law and felt competent to write on the subject; and the young Gibbon in Lausanne, who considered attending courses in law, was probably not unusual in the time he devoted to reading great legal treatises, refusing to be put off, as he noted in his autobiography, "by the pedantry of Grotius or the prolixity of Pufendorf."[16]

THE PROMINENCE OF LAWYERS in the formation of modern culture, with its characteristic attention to the workings of this world, is in some respects not altogether surprising. Lawyers were members of an articulate as well as a learned profession in which success required some discipline of mind and was likely to bring the wealth and leisure to support general reflection. Lawyers often had, too, the social status and influence needed to make their views heard. But I would suggest that something more was involved: that men of the law were uniquely fitted by their social role and the nature of their experience with the world to interpret it plausibly to contemporaries. Engaged to a special degree in the task of meeting the essential needs of a changing society, they were in a better position than other groups in Europe to give expression to society's changing perceptions. It is therefore necessary to look briefly at what these men were called upon to do and at what their function represented at the level of values.

That lawyers contributed substantially to the slow transition from medieval forms of political and social organization has long been recognized in a general way, and some description of this contribution in detail is now possible. As in so much else, precedents were supplied by the medieval Church, which, from the time of the investiture struggles, was increasingly administered by lawyers concerned to define its rights.[17] In the later Middle Ages most bishops, including those of Rome, were lawyers rather than theologians;[18] and the litigiousness of early modern Europe was prefigured by the litigiousness of the medieval Church,[19] where a sense of the inappropriateness of domination by lawyers nourished generations of reformers.[20] Two points may be made about the rise of lawyers in the

[16] Samuel I. Mintz, "Hobbes's Knowledge of the Law," *Journal of the History of Ideas*, 31 (1970): 614–15; Edward Gibbon, *The Autobiography of Edward Gibbon*, ed. Dero A. Saunders (New York, 1961), 102.

[17] See R. W. Southern, *The Making of the Middle Ages* (New Haven, 1953), 145–46, 151.

[18] On this situation, see Giuseppe Alberigo, *I vescovi italiani al Concilio di Trento (1545–1547)* (Florence, 1959), 55.

[19] See Robert Brentano, *Two Churches: England and Italy in the Thirteenth Century* (Princeton, 1968), 132–73.

[20] For a typical expression of this feeling, see the review of the college of cardinals by the Venetian ambassador to Rome, Paolo Tiepolo, in 1576, in Eugenio Albèri, ed., *Relazioni degli ambasciatori veneti al senato* (Florence, 1839–63), ser. 2, vol. 4, pp. 222–23: "Since there are many lawyers among them, because this profession is more highly valued at the Roman Curia than

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Church. In the first place their emergence into positions of power corresponded to the increasing importance of institutional controls accompanying the centralization of the ecclesiastical apparatus and the evolution of the Church into a mechanism for government. Here as elsewhere law became significant in direct proportion to the growth of social and institutional complexity. At the same time the prominence of lawyers reflected the growing acceptance of the inevitability of conflict even in the Body of Christ. For canon lawyers—and it should be noted that they were not necessarily ecclesiastics—were divided, both ideologically and practically, into antagonistic schools that represented conflicting principles and interests, and litigation in the Church regularly involved canonists on both sides of a dispute. The establishment of lawyers in the Church consecrated professional representation in adversary proceedings and accustomed men to rely on the expert services of the legal profession.

From the twelfth century, as towns grew in size and their societies, too, became more complex, the legal profession became increasingly important in the secular world, with the competence and respectability of lawyers nourished by the revival of Roman law. In the governments of the Italian towns, lawyers and notaries assumed responsibilities out of all proportion to their numbers, precisely because they possessed skills essential to the development of a more complicated social order. They drafted legislation in an age remarkably confident in the regulatory and reformatory value of laws, they participated in all public commissions, they staffed agencies of state, they went on embassies and prepared treaties, and they formulated and administered policy for republics and despotisms alike. Their interpretations of Roman law legitimized the sovereignty and independence of states.[21] And lawyers achieved equal prominence elsewhere in Europe, if somewhat more gradually, as societies beyond the Alps also grew more complex. The feudal conception of the king as dispenser of justice made his employment of lawyers appear natural even when their activities were resented. In France the administrative competence of lawyers was extended by the familiar institutional ambiguities of the Old Regime, in which, just as administrative agencies regularly performed some judicial tasks and thus needed lawyers, so the sovereign courts steadily expanded their administrative responsibilities in the name of the king. In the sixteenth century, for example, the Parlement of Paris took over hospital and university administration and also supervised the administration of the city. As *maîtres des requêtes* lawyers were indispensable to the councils of the king and managed a wide range of his affairs. Meanwhile lawyers supported royal authority by historical and constitutional argument and attacked the feudal establishment by both

any other, very few theologians are found there, indeed perhaps none, although theology ought to be the principal profession of priests."

[21] Martines, *Lawyers and Statecraft*, *passim*.

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their studies and their service in the courts.[22] In Spain, in the Netherlands, and in the Empire lawyers dominated royal councils and administration. Lawyers also ran colonial empires, both in bureaucracies at home and in the new societies developing overseas. A similar situation prevailed in England. Common lawyers molded the Star Chamber, promoted the Tudor revolution in government, and supplied personnel for every kind of administrative post. From Bosworth Field to the accession of Elizabeth every chancellor of the exchequer was a lawyer. So was every one of the twenty-two speakers of the House of Commons.[23]

The activity of lawyers in the construction of a new political order is well known. We know less, unfortunately, about the part played by lawyers in the shaping of social and economic life and about their representation of private clients. But it is clear that, on the Continent, lawyers had early developed the legal basis for the corporate structure of the society that persisted through the Old Regime out of the resources of Roman law.[24] As judges lawyers enforced the harsh demands of the criminal law; they stood for law and order and above all for the protection of property. Lawyers and notaries served the material security and perpetuation of families by drawing up marriage agreements and wills. They also ensured the performance of business agreements by contracts, deeds, and bills of sale; here they both met a practical need and provided the psychological prerequisite for economic activity. And eventually the common lawyers of England, some of them heavy investors in the great trading companies, were instrumental in freeing economic life from royal control.[25]

It is evident, at any rate, that lawyers were essential in developing the institutions and the conventions of early modern Europe. This was their peculiar task, and we can usefully linger for a moment on what this meant. Lawyers were needed to deal with the most urgent problems of their societies, not only incidentally and occasionally, like the majority of men, but in the most concentrated form imaginable, at every moment of their working lives. As private practitioners they saw and represented clients who were in trouble, feared trouble, sought the clarification of some ambiguous situation, or aimed to twist the social system in some novel and advantageous way. As servants of government lawyers were concerned to enforce

[22] Franklin L. Ford, *Robe and Sword: The Regrouping of the French Aristocracy* (Cambridge, Mass., 1953), 35; Shennan, *Parlement of Paris*, 86–93; Shennan, *Government and Society in France, 1461–1661* (London, 1969), 40; William Farr Church, *Constitutional Thought in Sixteenth-Century France: A Study in the Evolution of Ideas* (Cambridge, Mass., 1941); Kelley, *Foundations of Modern Historical Scholarship, passim*.

[23] Schwartz, "Magistracy and Society in Colonial Brazil," 716. For lawyer-bureaucrats in Spain, see also Richard L. Kagan, "Universities in Castile, 1500–1700," *Past and Present*, no. 49 (1970): 55–61. Ives, "Common Lawyers in Pre-Reformation England," 153–71.

[24] Martines, *Lawyers and Statecraft*, 12; Gaines Post, *Studies in Medieval Legal Thought: Public Law and the State, 1100–1322* (Princeton, 1964), ch. 1.

[25] This much seems clear, whatever the merits of the view of Coke as a precursor of liberalism. On the controversy over this issue, see the bibliographical essay in Little, *Religion, Order, and Law*, 238–46, and Little's own discussion, *ibid.*, 167–217.

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*Fig. 2, above.* A crowded sixteenth-century German courtroom. The judge with the symbolic sword of office is at the left. Title-page illustration from *Gerichtzordnung im furst'nthümb Obern und Nidern Bayrn Anno 1520 aufgericht* (Munich [1520]).

*Fig. 3, facing page.* A court in session, as depicted in a German lawbook of the early sixteenth century, *Der neu Layenspiegel von rechtmässigen ordnungen in Burgerlichen und peinlichen Regimenten* (Augsburg, 1511), fol. clxvi$^{v}$. The symbolic angel and devil on either side of the figure pleading before the court suggest that the proceedings are still viewed in a spiritual context, but note also the worldly faces of the jurists seated on both sides of the presiding judge.

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and extend the rights of central authority or to shape institutions that could do so more effectively, usually in an abrasive struggle with a hostile adversary; the personal costs of failure could be high. As judges they were compelled to scrutinize and weigh evidence, often of a disheartening and usually of an equivocal nature. Their role, in short, was to man the frontiers between the safe and familiar on the one hand, the dangerous and new on the other; between the tolerable and the intolerable; between the conventional world and the chaos beyond it. They constituted a kind of civil militia whose difficulties were compounded by the fact that the precise location of the frontiers to which they were assigned was rarely clear, and these frontiers were constantly changing. We may well ask what kind of men these were.

It is hardly remarkable that a special and rather unattractive temperament has been conventionally attributed to the lawyer.[26] He was, like Guicciardini, a skeptic and cynic; like Bacon, cold and crafty; or, like Bacon's great rival Coke, mean and harsh, a bully and a coward, a man of whom even his widow, after thirty-six years of marriage, remarked at his death, "We shall never see his like again—praises be to God." From the lawyer's exposed position on the frontiers of behavior he saw the world, like Montaigne, at its most irrational, most selfish, venal, and hypocritical. The lawyer knew the small practical worth of ideals and fine principles, and he knew also the humiliations and indignities that were the price of success in the world of men, a price that, like Bacon or Coke, even the best of lawyers was nevertheless prepared to pay.[27] The degradation that ended Bacon's public career nicely illustrated his own melancholy reflection on the hazards of success: "The rising into place is laborious, and by pains men come to greater pains; and it is sometimes base, and by indignities men come to dignities. The standing is slippery, and the regress is either a downfall or at least an eclipse, which is a melancholy thing."[28] In short, the lawyer knew the world. His mind and character were shaped to a unique degree by contact with its changing pressures and brutal realities, its dangers and uncertainties; and it is for this reason that he was peculiarly fitted to play so large a role in forming the culture of worldliness and vigilant individualism to which the more optimistic and trusting culture of the preceding period gradually gave way.

[26] Martin Mayer, *The Lawyers* (New York, 1966), 3, quotes Lord Melbourne: "All the attorneys I have ever seen have the same manner: hard, cold, incredulous, distrustful, sarcastic, sneering. They are said to be conversant with the worst part of human nature, and with the most discreditable transactions. They have so many falsehoods told them, that they place confidence in no one." See also Mayer's discussion of the change of personality even among law students, *ibid.*, 76–77.

[27] Martines, *Lawyers and Statecraft*, 112; Thorne, *Coke*, 4; Christopher Hill, *Intellectual Origins of the English Revolution* (Oxford, 1965), 225–26; Catherine Drinker Bowen, *Francis Bacon: The Temper of a Man* (Boston, 1963), 14–15, 31–32, 50.

[28] Francis Bacon, "Of Great Place," in his *Essays*, ed. Mary Augusta Scott (New York, 1908), 45–46. See also Bacon's poem on the misery of life given in John Aubrey, *Brief Lives*, ed. Oliver Lawson Dick (London, 1950), 10–11.

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*Fig. 4.* Lawyers selling justice to simple folk. This late fifteenth-century Spanish woodcut suggests something of the lay feeling of helplessness and suspicion in the presence of the mysteries of the developing legal system. From Aznar y García, *Indumentaria española,* estampo 15.

At any rate the lawyer commanded vast, if sometimes equivocal, respect. In Florence the guild of lawyers and notaries ranked first on all ceremonial occasions, and its chief officer was honorary head of the whole guild system. As individuals, doctors of law in Renaissance Italy ranked with knights.[29] Traiano Boccalini, a kind of Art Buchwald of the later Renaissance, argued that lawyers better deserved the title of Excellency than the nobles who complained of the lawyers' presumption.[30] In France the lawyer's profession was considered—except by some nobles—as honorable as bearing arms: La Bruyère described both functions as equally sublime and useful. The great Anglican theologian Richard Hooker, a devotee of law in a grander sense, declared that "soundly to judge of law is the weightiest thing which any man can take upon him."[31] Even the hatred and scorn

[29] Martines, *Lawyers and Statecraft,* 14, 29–30.

[30] Traiano Boccalini, *Ragguagli di Parnasso e scritti minori,* ed. Luigi Firpo (Bari, 1948), 1: 177–80.

[31] La Bruyère is quoted by Ford, in *Robe and Sword,* 70–71, n.44, though Ford observes that La Bruyère occasionally expressed other sentiments; for Hooker's comment see his *Of the Laws of Ecclesiastical Polity,* in his *Works,* ed. John Keble (Oxford, 1845), 1: 278.

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for lawyers so frequently encountered in every age in which they have been numerous are a tribute to their importance, if only through their capacity to threaten others; lawyers represented the omnipresent danger inherent in the increasingly complex and increasingly mysterious machinery of social organization, before which the individual felt more and more helpless. They clearly endangered the self-esteem of old nobles like Saint-Simon.[32] The deliberate exclusion of lawyers from More's Utopia only suggests their central place in the real world for one who understood it well.[33] And an oblique tribute of a similar kind may be discerned in Luther's frequent expressions of hostility to lawyers. One of the young men who sat at his table reported an incident of particular interest in this connection: "The doctor took his child in his hands and said, 'If you should become a lawyer, I'd hang you on the gallows. You must be a preacher and must baptize, preach, administer the sacrament, visit the sick and comfort the sorrowful.' "[34] The episode suggests the celebrated guilt of the youth who had abandoned law school, sold his lawbooks, and entered a monastery against the wishes of his own father. For Luther the law continued to represent the world and its spiritual dangers, which he had forsaken to pursue his salvation; lawyers remained, in his mind, at the center of this world's concerns. But this view was more than a private eccentricity. Luther's attitude also pointed to the degree to which the figure of the lawyer persisted in haunting the European religious consciousness. Indeed, this may have had as much to do with his unpopularity as the inconveniences he represented. The lawyer was an obvious scapegoat for the general guilt of a world in transition, made anxious not only by the immediate insecurities of life in society but also by the abandonment of old ways and values.

THE SIGNIFICANCE OF LAWYERS in European life depended on more than the importance of their practical role or the respect in which they were held. It rested on the deeper needs they satisfied. And here, to make the point more concrete, I will give particular attention to the period between the middle of the sixteenth and the middle of the seventeenth centuries. If the transition from the medieval to the modern world was generally characterized by a crisis of both social order and belief, this phase of the transition was peculiarly troubled by their convergence in religious wars, by international conflict of particular intensity, by inflation and depression, and by mounting social tension and dislocation.[35]

[32] See Ford, *Robe and Sword*, 72–73.

[33] More, *Utopia*, ed. Edward Surtz (New Haven, 1964), 114: "Moreover, they absolutely banish from their country all lawyers, who cleverly manipulate cases and cunningly argue legal points."

[34] *D. Martin Luthers Werke. Kritische Gesamtausgabe: Tischreden* (Weimar, 1912–21), vol. 2, p. 96, no. 1422.

[35] Hence the title of the essays collected from *Past and Present:* Trevor Aston, ed., *Crisis in Europe 1560–1660* (London, 1965).

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One consequence of the peculiar difficulties of this period was a singular exaltation of law as an antidote to disorder. The value attached to law now went far beyond the traditional and more measured confidence, as in the generation of Erasmus and Machiavelli, in the capacity of legislation to regulate and improve human conduct. In the frustrated Italian states of this period law presented itself as the only means to discipline the violent passions of mankind that had so obviously destroyed the freedom of Italy, and as the only instrument to control the wanton masses. To this tendency both the mood and the intellectual resources of the Counter Reformation contributed. In France law, beginning with Bodin, was now first clearly perceived as the essence of sovereignty, Gallican jurists dominated political discussion, reformers saw law as the solution to every moral and religious problem, and the high magistracy at last completed its slow evolution into a new nobility of the robe. In England men were finally compelled by rapid social and institutional change and the accompanying growth in legal business to take a fresh look at the common law, to adapt a legal system based on the needs of an older agrarian society to new social and political uses, and to reduce that legal system to some kind of order.[36] This was the greatest age of the common lawyers, whose numbers multiplied. Admissions to the Inns of Court generally doubled; Gray's Inn saw as much as a fivefold increase.[37] The printing press helped in all this, supplying uniform editions of laws that made legal systems everywhere more reliable, systematic, and effective.[38] During the same period the polity most admired in the whole of Europe was Venice, primarily because she gave the impression of an order based on sound and equitably administered laws.[39] In addition this period is of unusual interest for the comparative study of lawyers because lawyers were often peculiarly conscious of themselves as an international community of professional men that transcended political and confessional boundaries and was held together by common goals and ideals, common problems, and a common intellectual culture. An international correspondence among the lawyers of this age included the

[36] For the Italian states, see the remarks of Carlo Curcio, *Dal Rinascimento alla Controriforma: Contributo alla storia del pensiero italiano da Guicciardini a Botero* (Rome, 1934), 61; see also Rudolph von Albertini, *Das florentinische Staatsbewusstsein im Übergang von der Republik zum Prinzipat* (Bern, 1955); for France, see the remarks of Antoine Adam, *Du mysticisme à la révolte: les Jansénistes du XVII^e siècle* (Paris, 1968), 37; and Ford, *Robe and Sword,* 63; for England, see Hill, *Intellectual Origins,* 227–28; and Thorne, "Tudor Social Transformation."

[37] Stone, "Educational Revolution in England," 51–79. Thomas G. Barnes generously supplied me with his tabulation for Gray's Inn.

[38] Elizabeth L. Eisenstein, "Some Conjectures about the Impact of Printing on Western Society and Thought: A Preliminary Report," *Journal of Modern History,* 40 (1968): 14–15; Wilfrid Prest, "Legal Education of the Gentry at the Inns of Court, 1560–1640," *Past and Present,* no. 38 (1967): 24.

[39] This has been noted for England by John R. Hale, *England and the Italian Renaissance: The Growth of Interest in Its History and Art* (Oxford, 1964), 30; and for Florence by Albertini, *Florentinische Staatsbewusstsein,* 20–31, among others. For fuller documentation, see my "Venice and the Political Education of Europe," in J. R. Hale, ed., *Studies in Renaissance Venice,* scheduled for publication in 1973. Among those who admired the Venetian legal system was Jean Bodin, otherwise often critical of Venice.

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exchange of views on religious, literary, historical, and scientific topics as well as on the events of the day and discussion of legal questions of mutual concern.[40]

Much in the special importance attached to law in this period was a response to the unusual intensity of disorder in a Europe whose growing social complexity meant a new degree of vulnerability to dislocation and thus a growing need for regulation. The primary source of disorder was patently the conflict of human interests; and, as in the Church, the rise of the legal profession in secular society signified a concern for the peaceful resolution of conflict. Lawyers started to work when conflict loomed. A legal system may therefore be described as a means for institutionalizing conflict, but there was a crucial difference, at first subtle and disguised but increasingly radical and explicit, between medieval and modern assumptions about this process. Litigation in secular courts had been forbidden by the early Church; Augustine, who knew the courts well, had pointed to lawsuits to illustrate the persistent sinfulness of earthly society.[41] The notion of a relative natural law appropriate to man's fallen state had resolved the practical problems of life in a society that, although professedly Christian, remained imperfect.[42] But the acceptance of conflict was, in this view, also relative; it could never be tolerated as normal and inevitable. Justice was itself finally an absolute; and this meant that a legal decision was ideally concerned not so much to resolve conflict as to transcend and abolish it by resort to ultimate principles. In this light coercion by legal authority was an effort to bend the refractory wills of men into conformity with a final vision of justice. But increasingly the lawyers of early modern Europe, whatever the formulas to which they sometimes still appealed, disregarded such conceptions. Their task was practical and limited; they aimed not to transcend conflict but to manage it. In their world the essential tensions were not between sin and ultimate justice but between antithetical human interests that generally seemed morally ambiguous on both sides. Their activity was directed to the effective resolution of conflict, not to the realization of a lofty vision; their rise signified and accelerated the breakdown of a traditional view in which social values were defined in accordance with final ends. As early as the thirteenth century it was a commonplace that a good jurist made a bad Christian.[43]

Lawyers were thus central to the cultural transformation that marks

[40] See, for example, the correspondence of Paolo Sarpi with a group of French lawyers in his *Lettere ai Protestanti,* ed. Manlio Diulio Busnelli (Bari, 1931), and his *Lettere ai Gallicani,* ed. Boris Ulianich (Wiesbaden, 1961). The confessional distinctions reflected in these titles are irrelevant to the content of the letters and obscure the point that this was largely an exchange among lawyers. For Sarpi's contact with English legal circles, see Vittorio Gabrielli, "Bacone, la riforma e Roma nella versione hobbesiana d'un carteggio di Fulgenzio Micanzio," *English Miscellany,* 8 (1957): 195–250.

[41] Augustine *De civitate Dei* 19. 5.

[42] See the classic discussion of Ernst Troeltsch, *The Social Teachings of the Christian Churches,* tr. Olive Wyon (London, 1931), 1: 150–55.

[43] Victor Martin, *Les origines du Gallicanisme* (Paris, 1939), 1: 137–38.

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the end of the Middle Ages, and they took a leading part in the articulation of a novel set of empirical, pragmatic, and secular attitudes in which the orderly administration of this world's affairs was seen to depend on practical principles of its own. This new vision, which reached reasonable clarity only in the sixteenth century and still encountered deep resistance, was based on a perception of the world as an infinitely complex population of forces in conflict that, though ceaseless and terrifying, was nevertheless not altogether maleficent. A lawyer knew from experience that, in this world, constructive results sometimes came from the clash of forces and that to avoid conflict might, in social terms, invite worse consequences than to accept it. Like Paolo Sarpi, legal adviser to the Venetian Republic, he might therefore deplore an excessive pacifism, either in James I or at home.[44] As Sarpi wrote to a French lawyer, in one of the international exchanges now characteristic of the legal profession, the times needed a Democritus or a Heraclitus, a remark equally applicable to metaphysics and to politics in its sense of the world as a dynamic flux of particular entities in incessant interaction.[45] The positive value of confrontation between opposing forces is suggested at another level by Bacon's observation that "truth emerges more readily from error than from confusion."[46] Sarpi, who was also in touch with Bacon's circle, would have agreed with this early hint of the liberal principle that truth may be better conceived as a pursuit than a discovery and that it may be preferable to take a doubtful position than none at all.[47] Also suggested was the active life in which men are willing to engage with an adversary, however dubious the battle; and we are reminded again of the original connections between the law and the social vision of Renaissance humanism.

A lawyer's experience with the real world of unpredictable and hostile forces was likely to make him suspicious of those great systems of thought which, by presuming to take general account of all possibilities in advance, inhibited adaptation to changing circumstance and interfered with meeting the daily need for practical order. He needed first of all to sort out and scrutinize the discrete data in a case; his mind was characterized by what a distinguished modern jurist has ruefully defined as the capacity to "think about a thing inextricably attached to something else without thinking of the thing which it is attached to."[48] Thus the lawyers of our critical period were notable for their repudiation of systems and their preference for such limited sense as could be constructed from particular phenomena. Guicciardini's famous rejection of abstract speculation and

[44] William J. Bouwsma, *Venice and the Defense of Republican Liberty: Renaissance Values in the Age of the Counter Reformation* (Berkeley, 1968), 526–28.

[45] Paolo Sarpi to Jacques Leschassier, Sept. 14, 1610, in *Lettere ai Gallicani*, 93. The same pre-Socratic philosophers were also favorites of Bacon; see his *Novum Organum*.

[46] Bacon, *Novum Organum*, in his *Works*, ed. James Spedding (New York, 1869), 8: 210.

[47] See Sarpi's attitude to controversy in connection with Gallicanism, as in his letter to Jerôme Groslot de l'Isle, Mar. 13, 1612, in *Lettere ai Protestanti*, 1: 220–21.

[48] Thomas Reed Powell, quoted in Mayer, *The Lawyers*, 86.

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generalization[49] found a parallel in Sarpi's contrast between dialectic and law, in the emphasis of such French jurists as Pithou and Pasquier on the particularities of time, place, and circumstance, and in Bacon's contempt for Scholasticism and his opposition to the systematization of the law itself, except in chronological terms that would respect its particularity.[50] This helps to explain why the law, which the notary Salutati had championed against the deductive and humanly irrelevant science of the fourteenth century,[51] converged in the seventeenth with the new empirical science. Important lawyers in both France and England were fascinated, perhaps as much as physicians, by the achievements of scientists.[52] The same flexible and practical attitudes allied law with the—from another point of view lawless—doctrine of reason of state, which suggested the changing uses of the law, without regard to more ultimate considerations, in accordance with the changing needs of states.[53]

Similar attitudes informed the political vision of lawyers and shaped their loyalties; their rejection of the unworkable and irrelevantly general in favor of the immediately practical for their particular societies made them patriots. The lawyers of Venice resisted the interference of Rome, an interference that followed from a quite different conception of law, on the ground that the good order of Venice sufficiently demonstrated the excellence of her laws; these lawyers argued that the virtue of law was not absolute but relative to the needs of a particular society. Similarly French jurists from the time of Budé rejected the familiar claims of Roman superiority and insisted on the perfection of French law because it was so well adapted to French needs, and this legal nationalism was the point of departure for a more general cultural nationalism. In the same fashion the common lawyers of England praised English ways and the admirable consequences for England of the independence of English law from that of the Continent.[54]

[49] Directly applied to law in his *Ricordi*, ser. C, no. 111, in *Maxims and Reflections of a Renaissance Statesman*, tr. Mario Domandi (New York, 1965), 69: "Common men find the variety of opinions that exists among lawyers quite reprehensible, without realizing that it proceeds not from any defects in the men but from the nature of the subject. General rules cannot possibly comprehend all particular cases. Often, specific cases cannot be decided on the basis of law, but must rather be dealt with by the opinions of men, which are not always in harmony. We see the same thing happen with doctors, philosophers, commercial arbitrators, and in the discourses of those who govern the state, among whom there is no less variety of judgment than among lawyers."

[50] For Sarpi, see, for example, the first of his official *consulti* on the validity of the Venetian laws against which the papal interdict of 1606 was directed, in Paolo Sarpi, *Istoria dell'Interdetto e altri scritti editi ed inediti*, ed. Giovanni Gambarin (Bari, 1940), 2: 6: "It is not appropriate to proceed in these cases by way of conjectures, deductions, or syllogisms, but through explicit laws." For the French jurists, see Kelley, *Foundations of Modern Historical Scholarship*, 258, 279–80. For Bacon, see Shapiro, "Law and Science," 743 n.61.

[51] Salutati, *De nobilitate legum et medicine* (1399), discussed by B. L. Ullman in *The Humanism of Coluccio Salutati* (Padua, 1963), 31–32.

[52] Rousselet, *Histoire de la magistrature française*, 2: 247–50; Shapiro, "Law and Science."

[53] See Martines, *Lawyers and Statecraft*, 408–36.

[54] See my *Venice and Republican Liberty*, 449–52; for France, see Kelley, *Foundations of Modern Historical Scholarship*, 59–60, 288–90, *passim*; for England, see Ives, "Common Lawyers in Pre-Reformation England," for a review of this classic position.

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This position led to general praise of custom as the only satisfactory basis for law. So Pasquier made the case: "Since customs were formed gradually in each province according to the diversity of our characters, it seems appropriate in case of obscurity or doubt to have recourse to people close to us, who by their proximity would seem to conform to our manners and character and so to our customs."[55] The meaning of this proposition, for which there are analogues in the legal discourse of other places, was that custom best represented the proper particularity of the law, its necessary correspondence to the concrete and differing needs of peoples. The same point could be made about vernacular languages, and it is hardly surprising to find lawyers among their champions. Latin was customarily employed in a legal tradition that, whatever its practical tendencies, still professed to appeal to universal principles. The language of eternal Rome could always be counted on to give a subliminal ring of ultimate authority to a legal pronouncement: it implied finality where none could be claimed. Hence it is of particular significance that Pasquier advocated the use of French in the law as in other serious subjects, and Coke, though on occasion still inclined to take shelter in Latin, for the most part abandoned the tradition of earlier commentators on the law of his country to write in English.[56] Through changes in language, too, law was taking on a more practical and particular quality, and more was at stake than the greater ease of communication.

These various tendencies may be summarized in one word that will relate them to a development fundamental to every dimension of early modern European life: secularism—in the sense of a growing acceptance of the autonomy of the various aspects of human concern. Lawyers represented the growing assumption that life in the world is only tolerable when it is conceived as a secular affair and that the world's activities must be conducted according to manageable principles of their own rather than in subordination to some larger definition of the ultimate purpose of existence. By applying this assumption to solve the constantly changing problems of their societies, lawyers were, in a manner far more effective than that of any abstract philosopher, the supreme secularizers of their world. By imposing their own secularism on the machinery of social life they helped to accustom their contemporaries to think in secular terms, thus contributing in a fundamental way to the secularization of every other dimension of human concern: to the secularization of politics and economic life, of science, philosophy, and literature and the other arts.

At the same time it is important to recognize that secularism in this sense is not synonymous with unbelief. Some lawyers, like occasional members of other groups, may have been unbelievers; but more charac-

[55] Pasquier, quoted in Kelley, *Foundations of Modern Historical Scholarship*, 289. For similar attitudes elsewhere, see my *Venice and Republican Liberty*, 451–52, and Hill, *Intellectual Origins*, 250–51.

[56] Kelley, *Foundations of Modern Historical Scholarship*, 269, 272–73; Hill, *Intellectual Origins*, 258–59.

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teristic of the profession was, I think, a preference, shared by both Catholic and Protestant lawyers, for a kind of piety that stressed the spiritual and inward quality of the faith, contrasted it sharply with the world and its ways, and by emphasizing the incongruity liberated secular life from direct religious control. The lawyers and notaries of Renaissance Italy, the Gallican (and later Jansenist) magistrates of Paris, and the lawyers of early Stuart England, both Puritan and Anglican, all tended to an Augustinian spirituality.[57] Some of them doubtless felt a personal affinity to Augustine—himself the product of a legal culture—whose influence was so powerful in this period generally.[58] But this only suggests a deeper community arising, once again, out of a peculiar experience with life. A serious-minded lawyer was forced to recognize that the earthly city for whose affairs he was responsible could at best achieve only a contingent order quite different from that of the heavenly city, and in addition he was constantly reminded of the unregenerate state of humanity and the need for divine assistance. He knew, in short, the difference between law and grace.

For if the lawyer, as secularizer, was in some sense an agent of change, he also represented the need for order and gave expression to the conservative impulses of his age. In a period singularly troubled by the collapse of traditional ways of life and yearning for stability, he promised a measure of security, both for individuals and for society as a whole. His services gave a sense of security to his client; even as he threatened the security of his client's adversary he compelled him to have recourse to the law. The lawyer also supplied security for the future; his role was to foresee and provide against as many as possible of the dangers that might lie ahead, and thus it reflected both distrust of the future and, at the same time, some confidence in the ability of men to plan ahead and to control the unfolding of their earthly lives. Dependence on lawyers has, therefore, some value in revealing attitudes fundamental to changing cultural patterns exhibited in such comprehensive matters as trust, time, and human freedom; resort to lawyers implied the reverse of fatalism.

But legal systems functioned above all as a source of order for society in general. While they did so in an obvious sense through the punishment of wickedness and vice, I would suggest—though it would be difficult to demonstrate concretely—that they also performed a larger service for the

[57] For Italy see the richly suggestive work of Charles Trinkaus, *In His Likeness and Image: Humanity and Divinity in the Thought of the Italian Renaissance* (Chicago, 1970); for Gallicanism, see William J. Bouwsma, "Gallicanism and the Nature of Christendom," in Anthony Molho and John A. Tedeschi, eds., *Renaissance Studies in Honor of Hans Baron* (Florence, 1971), 818–21. The Jansenism of the French magistracy has long been recognized, but see Goldman, *Dieu caché*, and Ford, *Robe and Sword*, 87–88; for England, see Little, *Religion, Order, and Law*.

[58] Peter Brown, *Augustine of Hippo: A Biography* (Berkeley, 1967), 23, remarks of North Africa: "A legal culture, hard-headed and relentless, had proliferated in its new clerical environment. Viewed by an Italian bishop who knew him well and heartily disliked his theology, Augustine was merely the latest example of an all-too-familiar figure, the *Poenus orator*, 'the African man of law.'" For the Augustinian revival of the seventeenth century, see Henri Marrou, *Saint Augustin et l'augustinisme* (Paris, 1955), 172–76.

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*Fig.* 5. Judgment against witchcraft in the sixteenth century. From *Der neu Layenspiegel*, fol. clix$^{v}$.

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vast majority who were disposed to obey the law. Legal systems contributed subtly, in this way, to ending the peculiar restlessness of European society that had come to a climax in the earlier seventeenth century. By defining what was socially intolerable and by reshaping the official forms of social intercourse, laws and the men who worked with them must have gradually renewed that sense of limit in the social universe, so profoundly threatened by the crumbling of established conventions, without which life had become not only practically hazardous but in a deeper sense unsatisfactory. The *Crisis in Europe* of Trevor Aston's title was also a crisis of mind and spirit, of which we may take as typical symptoms Don Quixote's disorientation, Mersenne's anxieties about the fifty thousand atheists in Paris, *The Anatomy of Melancholy,* the moral anarchy of the Jacobean stage, and the prosecution of witches, in which lawyers also had their part to play. Lawyers helped to relieve the terrors in this world by supplying a social foundation on which some sense of the order and meaning of life could be reconstructed. Through the orientation of the masses of the law-abiding, they enabled their contemporaries once again to feel at home in a familiar world.[59]

Indeed, even the contribution of the lawyer to change was largely a consequence of his conservatism, of his acceptance of the established order of society and his determination to achieve his purposes within it. Whether in the maintenance of public order or in resolving conflicts of interest, he was above all concerned to make the social system work.[60] Yet this was likely to make him a conservative with a difference. For, since cases frequently failed to conform exactly to type and in fact often presented novel and ambiguous features, a lawyer's concern with workability was likely to operate against a complete rigidity of mind. If they respected the structures within which they worked, lawyers also required those structures to be flexible, responsible, and continuously useful. Like Montaigne, Montesquieu, Selden, Fielding, or Beccaria, lawyers sometimes presented themselves as reformers.

The attitudes of lawyers also underwent a gradual change as circumstances and demands on the law shifted; the later seventeenth century was to see some modification of the pragmatic and relativistic tendencies that had characterized the profession during the previous hundred years, but here, too, the law is revealed as a sensitive and leading participant in the larger movement of European culture. Thus the legal speculation of France in the classic age displayed a growing tendency to appeal once again to the timeless sanctions of reason and religion;[61] the idea of a general and

[59] See the discussion of the positive function of crime in Lewis Coser, *The Functions of Social Conflict* (Glencoe, 1967), 124–27.

[60] See Martines, *Lawyers and Statecraft,* 168–69, and Hill, *Intellectual Origins,* 255.

[61] William F. Church, "The Decline of the French Jurists as Political Theorists, 1660–1789," *French Historical Studies,* 5 (1967): 1–40. The gradual triumph of this tendency should also not obscure the fact that it had been developing slowly over many decades in both Italy and France; see Gilmore, *Humanists and Jurists,* 26–37, and Kelley, *Foundations of Modern Historical Scholarship,* 102–03, 137.

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prescriptive natural law came to dominate the law schools of Germany and the Netherlands; and even in England efforts were made to systematize the common law on philosophical principles, though Matthew Hale's great *History of the Common Law of England* (1713) also suggests the limits of English participation in this European movement.[62] But such changes suggest, too, how the legal mind was constantly prepared to mediate between the general and the particular, the ideal and the concrete, order and energy, ideas and life itself. The life of the law is potentially fraught with tension between these poles; and this also helps to explain the regular prominence of lawyers in the creation of modern culture.

AMONG THE POLARITIES with which lawyers had to live, one proved singularly fruitful for the transition from medieval to modern conceptions of the world and is also of particular interest from the standpoint of the historian: the tension between continuity and novelty, between the need of traditionalistic societies to feel at one with the past and the practical requirements of adaptation to change. Such tension, which may account for the prominence, from an early point, of men connected with the law in the composition of history, may be discerned as early as the thirteenth century, in the chronicle of the Paduan notary Rolandino, and in the fourteenth century in the work of the early humanist Albertino Mussato, another Paduan notary, and in the interests of the Venetian doge Andrea Dandolo, who collected legal and political documents and wrote a history of his republic.[63] In their writings we may already find hints of the secular emphasis, the cool political analysis, and the consciousness of change in human affairs that distinguish the great Florentine histories of Bruni, Poggio, Machiavelli, and Guicciardini, again men all more or less associated with the law. When we add the names of Sarpi, Bacon, and Clarendon to this list of lawyer-historians and note the role of the Scottish bar in establishing the great tradition of historical study in eighteenth-century Scotland,[64] it should be clear that we are in the presence of some special affinity. Law formed men with broad interests in human affairs who were skilled in the examination of evidence, experienced in the difficult task

[62] See the classic work of Otto von Gierke, *Natural Law and the Theory of Society, 1500 to 1800*, introd. and tr. Ernest Barker (Cambridge, 1934), and Leonard Krieger, *The Politics of Discretion: Pufendorf and the Acceptance of Natural Law* (Chicago, 1965). For the Netherlands, see also E. H. Kossmann, "The Development of Dutch Political Theory in the Seventeenth Century," in J. S. Bromley and E. H. Kossmann, eds., *Britain and the Netherlands* (New York, 1960), 91–110. Hale's *History of the Common Law of England* has just appeared in a new edition with a useful introduction by Charles M. Gray (Chicago, 1971). For the general point, see Shapiro, "Law and Science," 729–48.

[63] For Rolandino, see Girolamo Arnaldi, *Studi sui cronisti della Marca Trevigiana nell'età di Ezzelino da Romano* (Rome, 1963), 79–208; for Mussato, see Manlio Dazzi, "Il Mussato storico," *Archivio Veneto*, ser. 5, vol. 6 (1929): 357–471; for Dandolo, see Enrico Simonsfeld, "Andrea Dandolo e le sue opere storiche," *Archivio Veneto*, ser. 1, vol. 14, pt. 1 (1877): 49–149.

[64] Clive, "Social Background of the Scottish Renaissance," 231.

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of sorting out what might be germane to a problem from the irrelevant detail surrounding it, and trained to reconstruct events, as well as close to the constantly changing social and political scene.

But the most important consequences for historiography resulted from the application of historical interests to the law itself, a process that began with the notary Lorenzo Valla, whose close philological researches revealed that the Roman law was a product of time and circumstance, a historical artifact rather than a body of universally valid legal wisdom. And this vision, transmitted to a generation of French jurists by the teaching of Jacques Cujas, stimulated a national school of medieval studies with a novel interest in the development of law and institutions rather than in the wars and royal actions of traditional historical writing. Though this school produced no individual masterpieces, it extended the range of historical scholarship and displayed both a new methodological rigor and above all that secular feeling for the particulars of human experience otherwise characteristic of the legal mind.[65]

At the same time the lawyer-historians of the later Renaissance in France were no mere *érudits*. They intended the vision of the past constructed by their scholarly work to be useful; and in the peculiar nature of their historicism they were at one with Sarpi's researches in Church history and the English common lawyers' study of the medieval constitution. Lawyer-historians resembled each other in their common contribution to reconciling the conservative instincts of an age desperately concerned with order to the adjustments demanded by the times, through myths supported by the most sophisticated research Europe had yet witnessed. In this way lawyer-historians made a new order palatable and historical study attractive. Thus Venice defended her ecclesiastical and political independence against the pressures of the Counter Reformation by arguing that her admirable society rested on law and custom going back to the origins of the Republic; by contrast, lawyer-historians maintained, the juridical and administrative centralization demanded by Rome was of recent origin. Sarpi's *History of the Council of Trent* (1619) was a sustained demonstration of this essentially juridical point.[66] The lawyers of France employed similar arguments in defense of French customary against imperial Roman law and of the Gallican liberties against ecclesiastical Rome. In both the underlying issue was the right of self-determination, a matter subjectively important with the emergence of national feeling and of immediate practical significance since public order seemed to depend on the exclusion of any kind of foreign interference. Thus the researches of French lawyers demonstrated the value of custom for the particular needs of France and the thesis that the canons of the ancient Church had guaranteed the autonomy she now required. The study of his-

[65] See Kelley, *Foundations of Modern Historical Scholarship*, and Huppert, *Idea of Perfect History*, *passim*.

[66] See my *Venice and Republican Liberty*, 568–623.

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tory, especially legal history, revealed the subversion in the Middle Ages of a system admirably suited to contemporary needs.[67]

Much the same argument defended the English common law against the challenge of the royal prerogative. For Sir Edward Coke, too, the problems of contemporary society were to be solved by the recovery of an ancient heritage. "No subject of this realm," he wrote,

> but being truly instructed by the good and plain evidence of his ancient and undoubted patrimony and birth-right (though he hath for some time by ignorance, false persuasion, or vague fear been deceived or dispossessed) but will consult with learned and faithful councillors for the recovery of the same. The ancient and excellent laws of England are the birth-right and the most ancient and best inheritance that the subjects of this realm have, for by them he enjoyeth not only his inheritance and goods in peace and quietness, but his life and his most dear country in safety. (I fear that many want true knowledge of this ancient birth-right).[68]

The defects in the scholarship supporting this vision were not difficult to identify; Hobbes was critical of Coke on this score,[69] and by the later seventeenth century men were casting about for a better foundation for the social order than this mythical past. But Coke was psychologically on sounder ground than Hobbes in his sense of the kind of argument required by the times; like Sarpi, like the lawyers of France, Coke helped contemporaries to accept change by representing it as continuity. Serious consideration of some other basis for law and order, and therefore for human liberty and human rights, depended on a new political and cultural climate.

Nevertheless the myth of a recovered past proved a perennial resource of European culture, and it should be of particular interest to those who think of themselves as historians of modern Europe. The lawyers did not create the myth themselves; they merely applied to their own discipline a notion common to reformers of every kind for centuries. It is, of course, the myth of renewal that is at the heart of both the Renaissance and the Reformation. But because of the central importance of the law in the development of modern European society, the application of the idea in this domain gave to the conception of an initial perfection, decay, and recovery the broadest possible social resonance. It could now serve as the organizing principle for understanding the whole process of secular history. In dealing with lawyers, therefore, we are also looking at the origins of the idea of modern history itself.

[67] Kelley, *Foundations of Modern Historical Scholarship, passim,* and Bouwsma, "Gallicanism and the Nature of Christendom," 821–26.

[68] Coke, quoted by Hill, *Intellectual Origins,* 257–58. On the general point, see also Little, *Religion, Order, and Law,* 31, 201, and Pocock, *Ancient Constitution.* Pocock's book is particularly suggestive in its recognition of affinities between French and English thought.

[69] For example, in *A Dialogue between a Philosopher and a Student of the Common Laws of England,* ed. Joseph Cropsey (Chicago, 1971), 96; see also Little, *Religion, Order, and Law,* 179.

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# Politics and Patricide in Freud's *Interpretation of Dreams*

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THE UNRIDDLER OF riddles who found the key to the human condition in the story of Oedipus was also a lover of jokes. When at the age of forty-five he was finally given an associate professorship, the still unknown Dr. Freud reported the event to a friend in mock journalese. He described his promotion as a political triumph:

> The public enthusiasm is immense. Congratulations and bouquets keep pouring in, as if the role of sexuality had been suddenly recognized by His Majesty, the interpretation of dreams confirmed by the Council of Ministers, and the necessity of the psychoanalytic therapy of hysteria carried by a two-thirds majority in Parliament.[1]

It is a cheerful fantasy, very Viennese: political authority bends the knee to Eros and to dreams.

"Where he makes a jest, a problem lies concealed." In *The Interpretation of Dreams,* published two years before his jocular announcement, Freud had laid down his first principle of understanding the problems of dreams: "A dream is the fulfillment of a wish." At the time of writing, in 1902, he was collecting material to demonstrate that the same rule held true for jokes. "Sometimes," he added, "the jest brings the *solution* of the problem to light as well."[2]

Briefer versions of this paper were presented to the American Psychoanalytic Association, December 17, 1971, and to the Modern European Group of the Southern Historical Association, Novemer 19, 1971. Many of the ideas contained here were developed in discussion with Professor William J. McGrath of the University of Rochester, whose unpublished article, "Freud and Vienna: The Politics of the Brother Band," treats a problem complementary to this essay: fraternal rivalry and its relation to Freud's politics. The paper also reflects the author's debt of three decades to his friend Norman O. Brown, whose powerful imagination and generous critical intelligence have helped to shape the questions though not to set the limits of the search for answers.

[1] Freud to Wilhelm Fliess, Mar. 11, 1902, in Sigmund Freud, *The Origins of Psycho-analysis: Letters to Wilhelm Fliess, Drafts and Notes: 1887–1902,* ed. Marie Bonaparte, Anna Freud, Ernst Kris, tr. Eric Mosbacher and James Strachey (New York, 1954), 344.

[2] The opening observation was made by Goethe of J. G. Lichtenberg, Freud's favorite satirist. Freud quoted it approvingly in *Introductory Lectures on Psycho-analysis* (1915–16), in *The Standard Edition of the Complete Psychological Works of Sigmund Freud* (hereafter *SE*) tr. and ed. James Strachey *et al.* (London, 1953–64), 15: 38; *The Interpretation of Dreams* (1900), *SE,* 4: 121. For the whole problem, see *Jokes and Their Relation to the Unconscious* (1905), *SE,* 8.



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In his moment of elation at his promotion Freud did not simply lean back in satisfaction with the reward of his achievement. Instead he freed his fantasy to conjure up a wider paradise. Playfully he projected an ordered parliamentary polity united in support of his unorthodox science of Eros. His imagined parliament, which rallied a two-thirds majority to proclaim the necessity of the psychotherapy of hysteria was, of course, an exact inversion of the political reality of the time. The Austrian Reichsrat of 1902 had itself fallen so deep into political hysteria that it was unable to find a simple majority (let alone two-thirds) to legislate on anything.

We need not believe that political paralysis as such was a major concern to Freud by 1902. What gnawed at him was something both more and less specific: his relationship to the whole political system, including its academic components and consequences. Here it was that his joke fulfilled his wish—the wish to bring political authority to heel. "The jest brings the solution to the problem as well." In the play of Freud's wit the political powers were neither subverted nor dissolved but, on the contrary, miraculously harmonized, united in common recognition of the validity of his theories. Freud thus celebrated in fantasy a victory over politics, the side of human affairs from which he had expected most in youth and had suffered most in manhood.

In the same letter in which the new professor gaily trumpeted his triumph, he also sounded notes of doubt and guilt. Freud felt he could have had the professorship sooner if only he had pressed his own cause. "For four whole years, I had not put in a word about it," he wrote to his friend, Wilhelm Fliess. Only after he had completed *The Interpretation of Dreams* had he decided "to take appropriate steps" with his superiors. To do so, however, involved him in a moral dilemma, "to break with my strict scruples" against cultivating the powerful. Having taken the common-sense road of speaking up for the academic recognition that he felt to be his due, Freud found his success tainted by guilt. His professorship appeared to him ambiguous: on the one hand, it was a victory of his common sense; on the other, it seemed a surrender to hated authority. "I have learned that the old world is governed by authority, just as the new is governed by the dollar. I have made my first bow to authority."[3]

Where Freud's playful fancy had escalated his promotion into a political triumph, his conscience thus shrank it into a moral delict. Behind these contradictory responses of fantasy and conscience to his long-awaited moment of professional success lay Freud's life-long struggle with Austrian sociopolitical reality: as scientist and Jew, as citizen and son. In *The Interpretation of Dreams* Freud gave this struggle, both outer and inner, its fullest, most personal statement—and at the same time overcame it by devising an epoch-making interpretation of human experience in which politics could be reduced to an epiphenomenal manifestation of

[3] Freud, *Origins*, 342, 344.

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psychic forces. I shall try to extract from the book a few of the materials which illuminate the counterpolitical ingredient in the origins of psychoanalysis.

*The Interpretation of Dreams* occupied a special place in the mind and heart of its author. He regarded it both as his most significant scientific work, the foundation stone of his whole achievement, and as the work that brought him into the clear personally, giving him the strength to face a troubled life anew. The very structure of the work reflects this dual nature. Its surface organization is governed by its function as scientific treatise, with each chapter and section systematically expounding an aspect of dreams and their interpretation. To this scientific structure Freud explicitly subordinated the personal content of the book, designating the dreams and memories that constitute it only as "material by which I illustrated the rules of dream-interpretation."[4] Yet a closer look reveals a second, deep-structure of the work which, running from one isolated dream of the author to the next, constitutes an incomplete but autonomous subplot of personal history. Imagine Saint Augustine weaving his *Confessions* into *The City of God,* or Rousseau integrating his *Confessions* as a subliminal plot into *The Origins of Inequality:* such is the procedure of Freud in *The Interpretation of Dreams.* In the visible structure of the scientific treatise he leads his readers upward, chapter by systematic chapter, to the more sophisticated reaches of psychological analysis. In the invisible personal narrative he takes us downward, dream by major dream, into the underground recesses of his own buried self.

It is the second quest, a *recherche du temps perdu,* that must particularly interest the historian. By following the dreams simply in their order of presentation one becomes aware of three layers in a psychoarcheological dig: professional, political, and personal. These layers also correspond loosely to phases in Freud's life, which he presents in inverse temporal order in *The Interpretation of Dreams.* The professional one lies roughly in his present; the political in the period of youth and childhood. Deepest of all, both in time and in psychic space, the personal layer leads back to infancy and into the unconscious where infantile experience lives still.[5] Thus Freud's dreams serve as a thread of Ariadne which we can follow step-wise downward into the realm of instinct.

The elements that appear in the dream arrangement as three clear layers were also constituents of a wracking crisis Freud experienced in the 1890s. Professionally, the frustrations that had dogged him from the begin-

[4] Freud, *The Interpretation, SE,* 4: xxvi.

[5] The ordering of the dreams does not, of course, follow their actual chronology, nor that of Freud's self-analysis. For these chronologies the fundamental work has been done by Didier Anzieu, *L'Auto-analyse* (Paris, 1959). Nor does Freud analyze any of his dreams comprehensively in *The Interpretation.* Rather he uses them as components in the reconstruction of his own experience into a meaningful personal history that justifies both his life and his new science.

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ning of his career had by 1895 produced a bitterness verging on despair. Wishing to be a research scientist, Freud had early been forced by impecuniousness into the career of a physician. True, he had easily won a prized fellowship to Paris in 1885 and briefly thereafter an appointment to a university hospital where he could have clinical material for teaching and research. But the Vienna children's hospital with which he was associated for a decade after 1886 gave him little research opportunity and less prestige. Efforts to win for it the status of a university teaching clinic failed. The most galling indignity Freud had to suffer, however, was the failure to be given a professorship. His long wait—seventeen years in all where eight was the norm in the medical faculty—crowned his intellectual isolation, professional frustration, and social malaise with what appeared to be academic defeat.[6]

The wider context of Freud's professional frustrations was a seething atmosphere of almost continuous political crisis. During the last five years of the nineteenth century Austria-Hungary seemed to be serving, as one of its poets observed, as "a little world in which the big one holds its tryouts"—tryouts for Europe's social and political disintegration.[7] The Habsburg Empire was pulling apart at the seams internally as Europe was internationally: vertically, on nationality lines; horizontally, on class and ideological lines. Until the nineties the contending political forces had been the classic ones: liberal versus conservative. But now new claimants rose from the social deeps to challenge the old ruling groups. Out of the working class sprang socialism; out of the lower middle class and peasantry arose both virulent nationalism and Christian Socialism. Under the leadership of the skillful Christian Social demagogue, Karl Lueger, the anti-Semites in 1895 conquered Vienna itself, the bastion of liberalism. The forces of racial prejudice and national hatred, which the bearers of liberal culture, Jew and Gentile, had thought dispelled by the light of reason and the rule of law, re-emerged in terrifying force as the "century of progress" breathed its last. The experience not only broke the political power of the liberal middle class but also belied its expectations of history and undermined its confidence in its rationalistic culture.[8]

[6] Both the facts and the interpretation of Freud's slow medical and academic career have been the subject of heated controversy. The most comprehensive, often productively documented, attempt to defend the Austrian academic and bureaucratic authorities against the charge of hostility, injustice, and prejudice against Freud is that of Joseph and René Gicklhorn, *Sigmund Freuds akademische Laufbahn* (Vienna, 1960). Kurt R. Eissler redresses the balance in Freud's favor with further substantial research in his counter-polemic, *Sigmund Freud und die Wiener Universität* (Bern, 1966). On the length of Freud's wait for a professorship, see especially the latter work, 24–25, 181–83.

[7] "Dies Oesterreich ist eine kleine Welt,/In der die grosse ihre Probe hält." Friedrich Hebbel, quoted in Heinrich Benedikt, ed., *Geschichte der Republik Oesterreich* (Munich, 1954), 14.

[8] For the vertical (nationality) disintegration, Robert A. Kann, *The Multinational Empire* (New York, 1950), is the sturdiest general survey. Berthold Sutter focuses sharply on the nationality crisis of the late nineties in *Die Badenischen Sprachverordnungen von 1897* (Graz-Köln, 1965). For the rise of the New Right in its anti-Semitic aspect, see P. G. J. Pulzer, *The Rise of Anti-Semitism in Germany and Austria* (New York, 1964). The cultural character and conse-

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Sigmund Freud, by family background, conviction, and ethnic affiliation, belonged to the group most threatened by the new forces: Viennese liberal Jewry. Though not—or, more accurately, no longer—a political man, Freud watched with anxious interest the rise to power of the New Right both in Austria and abroad, especially in the France of the Dreyfus affair. Karl Lueger was his *bête noir;* Émile Zola, the novelist who championed Dreyfus, his political hero.[9]

Freud needed no specifically political commitment to make him feel the lash of resurgent anti-Semitism; it affected him where he was already hurting—in his professional life. Academic promotions of Jews in the medical faculty became more difficult in the crisis years after 1897. Freud reported in ironically elliptical bureaucratese the answer another Jewish colleague awaiting promotion had elicited from a high-placed official, that "in view of the present state of feeling, it was no doubt true that, for the moment, His Excellency [the minister of religion and instruction] was not in a position etc. etc. . . . [to ignore] denominational considerations."[10]

In response to his professional and political frustrations Freud retreated into social and intellectual withdrawal. He actually stepped down the social ladder, from the upper medical and academic intelligentsia to which he had gained access in the eighties to a simpler stratum of ordinary Jewish doctors and businessmen, who, if they could not assist or further his scientific pursuits, did not threaten or discourage him. In 1895 Freud joined B'nai B'rith, the Jewish fraternal organization, as a homey haven where he was accepted without question as a person and respected without challenge as a scientist.[11]

The more Freud's outer life was mired, however, the more winged his ideas became. He began to detach psychic phenomena from the anatomical moorings in which the science of his day had imbedded them. The speculative daring of his ideas, such as his theories of the sexual etiology of the neuroses, increased his estrangement from the very men who would have to support his professional advancement. Freud's intellectual originality and professional isolation fed upon each other.

The third dimension of Freud's crisis in the nineties was personal, centering on the death of his father. "The most important event, the most poignant loss, of a man's life," was Freud's evaluation. Whatever the gen-

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quences of the crisis are partially explored in Carl E. Schorske, "Politics in a New Key: An Austrian Triptych," *Journal of Modern History,* 39 (1967): 343–86; Schorske, "Politics and the Psyche in *fin de siècle* Vienna," *AHR,* 66 (1960–61): 930–46; and Schorske, "The Transformation of the Garden: Ideal and Society in Austrian Literature," *AHR,* 72 (1966–67): 1283–1320.

[9] Freud to Fliess, Sept. 23, 1895; Nov. 8, 1895; Feb. 9, 1898, in Freud, *Origins,* 124, 133, 245; Ernest Jones, *The Life and Work of Sigmund Freud* (New York, 1953–57), 1: 392–93.

[10] Freud, *The Interpretation, SE,* 4: 137.

[11] Freud, "Address to the Society of B'nai B'rith" (1926), *SE,* 20: 273–74; Freud to Fliess, Dec. 12, 1902, in Freud, *Origins,* 237. The sociology of Freud's friendships and associations has yet to be worked out in detail.

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eral validity of this statement it certainly held true for Freud. His father's death in 1896 came at a time when it could only reinforce Freud's other difficulties. His dreams and their analysis make plain that his psychological crisis over his father's death unfolded as a crisis of professional failure and political guilt. To lay his father's ghost Freud had either, like Hamlet, to affirm the primacy of politics by removing what was rotten in the state of Denmark (a civic task) or to neutralize politics by reducing it to psychological categories (an intellectual task).

LET US NOW TURN TO *The Interpretation of Dreams* to see how Freud's threefold crisis and his scientific work were related. In chapter 2, the first substantive chapter of the book, Freud developed his basic analytic principle, that a dream is the fulfillment of a wish. He did so by personal means, constructing an extensive model analysis with a single dream of his own, the Dream of Irma's Injection. Though well aware of its many dimensions, Freud here interpreted the Irma Dream rather narrowly, in terms of the first circle of his hell: professional frustration and self-doubt.[12] This was the circle least likely to encounter resistances in his readers. In chapter 4 Freud proceeded to refine and hence redefine his first principle to read: "A dream is a *disguised* fulfillment of a *suppressed* wish." Again he selected a dream of his own for demonstrative purposes, the Dream of the Uncle with the Yellow Beard. On its innocent, nonsensical surface the dream said nothing, but its analysis showed Freud the unseemly moral consequences ensuing from the thwarting of his professional ambition by politics. His dream-wish was for the power that might remove his professional frustration. As he described it, the dream contained the political wish to "step into the minister's shoes," where he could eliminate his competitors and promote himself to a professorship.[13] The dream also revealed a disguised wish either not to be Jewish or to have the power to eliminate Jewish rivals. A political ambition functioned here as a means to professional self-realization; or, seen analytically in terms of Freud's psychoarcheological dig, a political wish was found to lie as a deeper reality beneath the professional one.

To explain the principle of distortion which he had found to govern the Uncle Dream, with its latent political wish, Freud appropriately introduced political analogies. The dream-thought, he suggested, confronts the same problem in the psyche of the dreamer as "the political writer who has disagreeable truths to tell those in authority." If the censor is strong

[12] Erik H. Erikson has enlarged and deepened Freud's analysis of this aspect of the dream with the concept of the generativity crisis of middle age in "The Dream Specimen of Psychoanalysis," *Journal of the American Psychoanalytical Association*, 2 (1954): 5–56. The most comprehensive and structured analysis is by Anzieu, *L'Auto-analyse*, 24–45. For a broad and rich but less rigorous treatment and for further bibliography on this and other dreams, see Alexander Grinstein's useful work, *On Sigmund Freud's Dreams* (Detroit, 1968), 21–46, *passim*.

[13] Freud, *The Interpretation*, *SE*, 4: 134–41, 191–93.

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the writer must "conceal his objectionable pronouncement beneath some apparently innocent disguise." Just as there are two social powers, ruler and people, Freud argued, so there are two psychic forces: "One of these forces constructs the wish which is expressed by the dream, while the other exercises a censorship upon this dream-wish and by the use of censorship, forcibly brings about a distortion in the expression of the wish." The social model provided an analogy for Freud to show us "a quite definite view of the 'essential nature' of consciousness."[14]

In his selection of the analogy, as in the elucidation of the Uncle Dream, we can see how the political reality of the nineties, through the issue of the professorship, had penetrated Freud's psychic life. In the Irma Dream analysis Freud gave his readers only his sense of personal professional impotence, easily deducible from the manifest content of the dream. In the Uncle Dream he pushed through the opaque surface of the manifest content to find politics in the latent content. Recounting the background of the Uncle Dream, Freud stated that the information concerning the "denominational considerations" which blocked his promotion, although "not news to me, was bound to strengthen my feeling of resignation."[15] He did not make explicit what his analysis of the Uncle Dream shows us: that much as he might cultivate political resignation in his waking life, the wish to free himself from anti-Semitism reasserted itself in his dreams. And even there, the power of the "secondary force"—the censor representing the social reality—distorted the dreamer's wish for liberation from the fate of the Jews into denigration of, that is, aggression against, his Jewish friends and colleagues.

In his third extensively analyzed dream (the Dream of the Botanical Monograph) Freud's father entered the picture through two recollected episodes that did little credit to the character of Jakob Freud. In one episode the father gave his little boy a book to destroy, "not easy to justify from an educational point of view!" In another, the father upbraided his adolescent son for extravagant book buying.[16] Father Jakob Freud thus first trod the boards in the Dream Book in the unpromising role of an anti-intellectual, frustrating little Sigmund the future scientist—as, more recently, the political world had done.

With this finding from childhood Freud adumbrated the next problem in his scientific exposition, the importance of infantile experience in dream life. In chapter 5, under the heading "Infantile Material as the Source of Dreams," we find, ironically enough, that Freud has concentrated most of the important political materials—memories and dreams—contained in *The Interpretation of Dreams*. On the hunt for the source

[14] *Ibid.*, 142–44.
[15] *Ibid.*, 137.
[16] *Ibid.*, 169–73, especially 172–73. Freud does not analyze at this point the meaning of these episodes, though his language clearly reflects resentment at his father's behavior.

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of his own "pathological ambition,"[17] Freud opened the gates of memory on childhood and early youth. As he did so, a political flood surged in.

Childhood experience, whatever universals may govern it, must be lived in a specific cultural milieu. The one that Freud recovered in self-analysis was that of newly triumphant liberalism in the Austria of the 1860s. He recalled his father's enthusiasm for the new Liberal ministers of 1867: "We illuminated the house in their honor." As his mind reached back "from the dreary present to the cheerful hopes of the days of the *Bürgerministerium*," Freud remembered how "every industrious Jewish schoolboy carried a minister's portfolio in his knapsack." A wandering poet in the Prater prophesied of young Sigmund—to the delight of his parents one would presume—that he would be a cabinet minister.[18] Indeed, until the very end of his Gymnasium years, Freud planned—undoubtedly, given his father's values, with paternal encouragement—to study law, the royal road to a political career. His ambitions were further strengthened by the school friend he most idolized, Heinrich Braun. Then a militant German democrat, Braun later became one of Central Europe's most prominent socialist intellectuals.[19]

In such a context of clear and confident mid-century liberalism Freud acquired the political values he retained all his life: partisanship for Napoleon as conqueror of backward Central Europe; contempt for royalty and the aristocracy (in 1873 as a senior in the Gymnasium Freud had proudly refused to doff his hat to the emperor); undying admiration for England, particularly for the great Puritan, Oliver Cromwell, for whom Freud, the sexual liberator, named his second son; and above all, hostility to religion, especially to Rome.

Having recovered the liberal enthusiasms of his boyhood in his dream analysis and reminiscing on its bright political hopes, Freud suddenly introduces the reader of *The Interpretation of Dreams* to what can only be called his Rome neurosis.

Like most cultivated Austrians of his generation Freud was steeped in classical culture. Once he hit upon the analogy between his work as depth psychologist and the work of an archeologist, his mild interest flowered into a burning passion for antiquity. He consumed with avidity Jakob Burckhardt's newly published *History of Greek Culture*, so rich in materials on primitive myth and religion. He read with envy the biography of Heinrich Schliemann, who fulfilled a childhood wish by his discovery of Troy. Freud began the famous collection of ancient artifacts which were soon to grace his office in the Berggasse. And he cultivated a new friendship in the Viennese professional elite—especially rare in those days of

[17] *Ibid.*, 192.

[18] *Ibid.*, 192–93.

[19] Freud to Julie Braun-Vogelstein, Oct. 30, 1927, in Sigmund Freud, *Letters of Sigmund Freud*, ed. Ernst L. Freud (New York, 1964), 378–80; Julie Braun-Vogelstein, *Heinrich Braun* (Stuttgart, 1967), 20–24.

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withdrawal—with Emanuel Löwy, a professor of archeology. "He keeps me up till three o'clock in the morning," Freud wrote appreciatively to Fliess. "He tells me about Rome."[20]

At first a hobby pursued for relief of tension, Freud soon found that Rome had acquired the character of a neurotic symptom. He was seized by an irrepressible longing to visit Rome. When he was stalled in his work on the dream book in 1898, he could do nothing but study the topography of Rome, "for which the longing grows ever more torturesome."[21]

Five times Freud traveled to Italy between 1895 and 1898, without ever reaching Rome. Some inhibition held him back. At the same time Rome became, literally, the city of his dreams. In *The Interpretation* Freud reports four Rome Dreams, all of which suggest, in one form or another, redemption or fulfillment that is never quite achieved.[22] Even the manifest content of these dreams speaks worlds. Freud conflates dream images of Catholic Rome with Jewish ideas and situations. In one dream Rome appears as "the promised land seen from afar," implying Freud to be in the same relation to Rome as Moses to Israel. The vision, though Freud does not say so, seems to express a forbidden wish: a longing for an assimilation to the gentile world that his strong waking conscience—and even his dream-censor—would deny him. He also identifies Rome with Carlsbad, Bohemia's equivalent of our Palm Springs, a city of pleasure, rest, and cure; in short, an earthly city of recreation (re-creation), of resurrection. Freud compares himself in the analysis of this dream to a poor, gentle Jewish character in one of the Yiddish stories he loved so well. Because the little Jew did not have the train fare to Carlsbad, the conductor beat him up at every station; but, undaunted, he continued on his *via dolorosa* (the expression is Freud's). Thus the lofty vision of Moses-Freud seeing Israel-Rome "from afar" had its lowly analogue in the picture of the little-Jew-Christ-Freud reaching Carlsbad-Rome on a *via dolorosa.* A third dream reinforces the Christian theme but telescopes it into that of ancient, pagan Rome. From a train window Freud sees across the Tiber the Castel Sant'Angelo, at once papal castle and Roman imperial tomb. Tantalizingly, the train moves off before he can cross the Bridge of the Holy Angel to reach the castle—a house of both buried paganism and Christian salvation.

[20] Freud to Fliess, Jan. 30, 1899; Feb. 6, 1899; May 28, 1899; Nov. 5, 1897, in Freud, *Origins,* 275–76, 282, 229. Suzanne Bernfeld has explored, with great sensitivity to cultural factors, the function of archeology both in Freud's scientific thought and, psychoanalytically, in his personal attempt to overcome guilt over death wishes. See her "Freud and Archeology," *American Imago,* 8 (1951): 107–28.

[21] Freud to Fliess, Dec. 3, 1897; Oct. 23, 1899, in Freud, *Origins,* 236, 269. The latter date, on which he reported his "ever more tortured longing (*die Sehnsucht immer quälender . . .*)" [retranslation mine], was the anniversary of his father's death.

[22] Freud, *The Interpretation, SE,* 4: 193–98. One later Rome Dream, in which the city is the setting of grief, is not included here. This dream's bearing on Freud's problem of ambivalence as Jew has been interestingly demonstrated by Peter Loewenberg in "A Hidden Zionist Theme in Freud's 'My Son, the Myops . . .' Dream," *Journal of the History of Ideas,* 31 (1970): 129–32.

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Freud does not analyze these dreams fully in *The Interpretation*. While recognizing that the wish to go to Rome "had become in my dream a cloak and symbol for a number of passionate wishes," he discloses fully only one of them. The clue to it he finds in Hannibal: "Like him, I had been fated not to see Rome."[23] This idea leads Freud to the recovery of a childhood scene in which he finds part of the source of his Rome neurosis. In that scene political obligation and oedipal aggression converged.

When Sigmund was ten or twelve years old (1866–68), his father wanted to illustrate how much the triumph of liberalism had improved the lot of the Jew. He told his son how, in an earlier time, he had been publicly humiliated by an anti-Semitic ruffian, "a Christian," as Freud pointedly calls him. Freud discovered upon questioning that his father had offered neither protest nor resistance to the indignity. Little Sigmund was disgusted with his father's "unheroic behavior." He contrasted his situation with another "which fitted my feelings better: the scene in which Hannibal's father . . . made his boy swear before the household altar to take vengeance on the Romans."[24]

"To take vengeance on the Romans": it was both pledge and project. And as project it was at once political and filial. In most other great creative Viennese who were Freud's contemporaries the generational revolt against the fathers took the specific historical form of rejection of their fathers' liberal creed. Thus Gustav Mahler and Hugo von Hofmannsthal both turned back to the baroque Catholic tradition. Not so Freud, at least not consciously. He defined his oedipal stance in such a way as to overcome his father by realizing the liberal creed his father professed but failed to defend. Freud-Hannibal as "Semitic general" would avenge his feeble father against Rome, a Rome that symbolized "the organization of the Catholic Church" and the Habsburg regime that supported it.[25]

We notice at once, of course, that the Rome of Freud the boy in the sixties—forbidding, hostile, bureaucratic—is quite different from the Rome in the dreams and longings of Freud the man in the nineties. The first is an object of hate, an enemy to be conquered, the second an object of desire, to be entered in love. Freud says nothing directly about the difference or the relationship between the two. But he offers a clue when he recalls a German classical author's question: "Which of the two [men]

[23] Freud, *The Interpretation, SE,* 4: 196–97.

[24] *Ibid.,* 197.

[25] That the Habsburg Empire in a wider sense was involved in Freud's boyhood militancy against Christianity is suggested in his identification of Hannibal with Napoleon—"both crossed the Alps"—and in his hero worship of Napoleon's Marshal Masséna. Little Sigmund learned of the latter from Louis Adolphe Thiers's *History of the Consulate and the Empire,* "one of the first books I got hold of when I had learnt to read." Masséna, whom Freud erroneously believed to be Jewish and who was born one hundred years to the day before Freud, was his "declared favorite" before he encountered Hannibal. Freud, *The Interpretation, SE,* 4: 196–98. Masséna not only fought the Catholic forces in Italy but occupied Vienna, making his headquarters in the Leopoldstadt (later the Jewish quarter in which Freud grew up).

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. . . paced his study in greater excitement after forming his plan to go to Rome: Winckelmann or Hannibal?" Freud unhesitatingly identifies himself with Hannibal, following in his footsteps of failure. "Like him, I had been fated not to see Rome."[26] Here Freud conceals an important truth from us, if not from himself, concerning his problem of political guilt as scientist and son. The Rome of his mature dreams and longings is clearly a love-object.[27] That is not Hannibal's Rome but Johann Joachim Winckelmann's. The great eighteenth-century archeologist and art historian saw in Rome the mother of European culture. A Protestant, Winckelmann overcame his scruples and embraced Catholicism in order to enter Rome and pursue his passion for classical antiquity as a papal librarian. He conquered his conscience for the sake of his science, his *amor intellectualis* for Rome.[28]

Winckelmann or Hannibal? Scientist or politician? Freud had faced such a choice before, in 1873, when he changed his career plans in high school. Intoxicated with Goethe's erotic description of Mother Nature, young Freud had decided to enter the university in science instead of law, thus following in the footsteps of Winckelmann the scientist—a "soft" scientist like Freud. In so doing, he had abandoned Hannibal's political mission.

In the nineties, as Freud said, "the increasing importance of the effects

[26] Freud, *The Interpretation, SE,* 4: 196.

[27] This Rome Freud connected (though not in *The Interpretation)* with his oedipal tie to his Czech Catholic nanny, who had introduced him to Catholicism and had given him "a sense of my own powers," in contrast with the discouragements of his Jewish father. Cf. Freud to Fliess, Oct. 3–4, 1897; Oct. 15, 1897, in Freud, *Origins,* 219–22. Psychoanalytic literature, following Freud, has tended to accept his primary identification of the Rome longing with the nanny as mother-substitute and oedipal love-object, reducing the Catholic and Czech attributes of Rome in Freud's dream-pictures to symbols of this primal tie, and the inhibition preventing travel to Rome as incest taboo. See, for example, Grinstein, *Freud's Dreams,* 75–76, 90–91; Jones, *Life,* 1: 5–6; Bernfeld, "Freud and Archeology"; and Kenneth A. Grigg, "The Role of the Nursemaid in Freud's Dreams: 'All Roads Lead to Rome,'" unpublished article. By emphasizing here the historical significance of the Jewish-Catholic tension in the dreams' manifest contents, I am trying to restore Freud's political-cultural experience to its dynamic, formative role in the development of his psychoanalytic system of thought in which, in effect, he resolves the pain of general history by translating it into personal history.

[28] "It is the love of science and it alone," wrote Winckelmann, "that can move me to give ear to the proposal suggested to me." Quoted in Carl Justi, *Winckelmann und seine Zeitgenossen* (5th ed.; Cologne, 1956), 1: 371. The first edition of this classic biography appeared during Freud's Gymnasium years. The second edition was published in 1898, when Freud's interest in archeology was at its height and when he had resumed work on the interpretation of dreams, inclusive of the Rome dreams. The Justi biography reveals remarkable similarities between Winckelmann's life and intellectual stance and Freud's: poverty, an acute sense of low social status, failure to find a congenial intellectual position or adequate professional recognition, a string of intense friendships with homosexual overtones, hatred of political tyranny, hostility to organized religion, and a generativity crisis at the age of forty that resulted in a "first work" of a new and revolutionary kind. Most of these features emerge clearly in Goethe's perspicacious essay of 1805, "Winckelmann," in *Goethes Werke,* ed. Eduard von der Hellen (Stuttgart, 1922), 15: 65–93. Herder, in a somewhat more romantic essay, "Denkmal Johann Winckelmann," in *Herders Sämtliche Werke,* ed. Bernhard Suphan, 8 (Berlin, 1892): 437–83, appreciates Winckelmann as a serene and stoical hero of science in an age of prejudice and rulers' stupidity. Freud would have seen himself in this version even more clearly than in Goethe's. Though Freud had a magnificent command of the German classics, I have been unable to determine whether he actually read any of these books, but the fact that the Hannibal-Winckelmann contrast played a key role in his analysis suggests familiarity with Winckelmann's character and aims.

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of the anti-Semitic movement upon our emotional life helped to fix the thoughts and feelings of those early days."[29] The ghosts of Hannibal and his father rose to call again for "vengeance on the Romans." They barred Freud's road to Winckelmann's Rome—the Rome of pleasure, maternity, assimilation, fulfillment. Science would have to defeat politics and lay the father's ghost.

THESE OBJECTIVES Freud accomplished with the help of what he called "A Revolutionary Dream." The description and analysis of this dream occupies the central position in Freud's demonstration of the principle that a childhood wish is the fundamental determinant of the meaning of dreams.[30]

It was in August of 1898 that Freud dreamt the Revolutionary Dream, and politics hung especially heavy in the air. After a winter of violence between Czechs and Germans, especially in the universities, the thorny problem of language rights remained unresolved. Parliament was still paralyzed, for the German parties refused to abandon obstructionist tactics until the government should revoke the language ordinances favoring the Czechs. In June there had been violent anti-Semitic outbreaks in Galicia. In addition to these difficulties there was the nagging, acute problem of renewing the 1867 accords to regulate the economic and fiscal relations between the Austrian and Hungarian halves of the Dual Monarchy.

Count Franz Thun, Austrian minister president since March 7, 1898, was devoting most of his summer to negotiations of a preliminary accord between the Austrian and Hungarian cabinets—negotiations that had to reckon with the prospect of resistance from both German and Hungarian nationalists in their respective parliaments. Thun himself was an arch-aristocrat, a great landholder and head of the party of the Bohemian high nobility; in short, a feudal political bureaucrat of the old school. Though he aimed at compromise with the Germans, Count Thun was goaded by their aggressiveness into tough measures against them and quickly became the object of their cordial hatred.[31] This was the figure who became Freud's central antagonist in the Revolutionary Dream.

Ironically enough, 1898, year of paralysis and chaos, was also being celebrated as the fiftieth anniversary of Emperor Francis Joseph's accession. Since the emperor had been brought to power in the revolution of 1848, that upheaval, too, was recalled to public mind, and to Freud's consciousness.[32]

On the day of his Revolutionary Dream, Freud was setting out for a

[29] Freud, *The Interpretation, SE,* 4: 196.

[30] *Ibid.,* 4: 208–19; 5: 431–35. My analysis of this dream is far from complete. For other dimensions, see Grinstein, *Freud's Dreams,* 92–160.

[31] Richard Charmatz, *Oesterreichs innere Geschichte von 1848 bis 1907* (2d. ed.; Leipzig, 1912), 2: 128–32.

[32] Freud, *The Interpretation, SE,* 4: 211.

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holiday with his family.[33] While he was waiting for his train at Vienna's Westbahnhof, he recognized Count Thun stalking on to the platform. The count was, as Freud correctly surmised, bound for the emperor's summer retreat in Ischl, where the preliminary Austro-Hungarian economic accords—the so-called Ischl clauses—were being worked out. In his bearing as in his politics Count Thun was a "*Feudalherr* from top to toe." "Tall, thin, dressed with exquisite elegance, [he looked] more like an Englishman than a Bohemian," one of his subordinates recalled. "His monocle never left his eye."[34] Now, as he passed through the train gate, the count displayed his aristocratic aplomb. Though he had no ticket he waved the ticket taker aside and entered a luxurious compartment. All Freud's resentment of aristocracy welled up at the minister's imperious behavior. He found himself whistling a subversive air from Mozart's *Marriage of Figaro:* "If the Count wants to dance, I'll call the tune."

The dream on the train bore the stamp of this chance encounter and the emotions it aroused. It telescoped Freud's present political feelings with his past political experiences and scenes and images from history. In the opening scene of the dream Freud found himself at a university student gathering, where Count Thun (or a conservative predecessor, Count Taaffe) was belittling German nationalism. In his disdainful way the aristocrat deprecated the symbolic flower of the German nationalists as a limp plant, colt's foot [German, *Hu*flat*lich*], that Freud connected by verbal association with flatulence; in short, the speaker implied that German student militancy was so much wind. Freud, to his own surprise, rose in angry response to the minister's contemptuous remarks. In analyzing the scene he identified himself with Adolf Fischhof, a medical-student leader who helped to trigger the revolution of 1848 at the university and to carry it on to the larger political stage.[35] Freud discovered another Jewish medical politico in the dream: his fellow student, Victor Adler. By 1898 Adler had become the leader of Austrian Social Democracy. In analyzing the Revolutionary Dream Freud recalled having defied Adler, toward whom he had had strong feelings of envy and rivalry, in a German nationalist student organization to which they both belonged in the 1870s.[36] Fischhof and Adler had shown that one could be both a Jewish doctor and a political leader—a fact that Freud virtually denied to be possible in explaining his choice of careers.[37] Even as Freud dreamt out his own long-

[33] McGrath, "Freud and Vienna," has established the date as August 11, 1898.

[34] Rudolf Sieghart, *Die letzten Jahrzehnte einer Grossmacht* (Berlin, 1932), 35.

[35] Like Freud, Fischhof was a poor, Moravian-born Jew. He became a doctor because before 1848 no other academic course was open to his race. When the revolution broke, Fischhof occupied a medical position later held by Freud: that of *Sekundararzt* (intern) at the Royal Imperial General Hospital in Vienna. Cf. Richard Charmatz, *Adolf Fischhof* (Stuttgart, 1910), 14, 17–31.

[36] Martin Grotjahn, "A Letter by Sigmund Freud with Recollections of His Adolescence," *Journal of the American Psychoanalytic Association,* 4 (1956): 649–52.

[37] "A ministerial career is definitely barred to a medical man," wrote Freud of his decision to abandon the law in 1873. *The Interpretation, SE,* 4: 193.

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repressed political wish and exhumed by analysis the figures who proved that his youthful dream of a political role might have been true, he refrained from putting the pieces together either for his reader or in all likelihood for himself.

In the scenario of the dream, after his outburst of anger at the minister, Freud suddenly fled the political scene. He retreated through the halls of the university, that is, through academia. Escaping into the street, he then tried to get out of town, to some destination where "the Court would not be in residence." The final scene accordingly was at the railroad station where, in his waking life, it had all begun. There Freud found himself on the platform in the company of a blind man, whom he recognized in his analysis as his dying father. Freud held a urinal for the helpless old man, conscious that the ticket taker would look the other way. So ends the dream.

With this scene, the dream had done its work in dissolving political impulses and political guilt. Before the dream, Freud had confronted the powerful Count Thun on the platform as a prerevolutionary Figaro with the subversive wish to call the tune for the count. His present situation of political impotence and resentment dominated this waking fantasy. In the dream he had discharged, by his defiance of the count, the commitment of his youth to antiauthoritarian political activism, which was also his unpaid debt to his father.

It is easy for the modern reader to forget the boldness—half nerviness, half courage—involved in Freud's presentation of the Revolutionary Dream and its daytime prelude to his contemporaries. Count Thun, after all, still headed the government when Freud sent the last pages of his manuscript to the printer in early September 1899. In the Revolutionary Dream, his last explosive hail-and-farewell to politics, Freud took the field as liberal-scientific David against a very real political Goliath, the incumbent minister president. He laid bare his political and social feelings in no uncertain terms. Yet both on the station platform and in the dream, the encounter between the little Jewish doctor and the gaunt aristocrat suggests a quixotic duel, at once heroic and absurd. In it Freud found by analysis not so much his civic courage as an "absurd megalomania, which had long been suppressed in my waking life."[38] The strange encounter, Freud versus Thun, had brought it to the surface.

Neither the dream nor Freud's reading of it came to rest in affirmation or rejection of a particular political position, not even that of Count Thun. Freud implied his own ideological truth when he saw himself in his dream as rejecting both aristocratic authority (Thun-Taaffe) and the authority of socialism (Adler, the older and bolder brother). The "political" problem was dissolved in the final scene on the platform, where the dream-thought substituted the dying father for the living count. Here the

[38] *Ibid.*, 215.

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flight from politics, through the university, and on to scientific medical service, found vindication. Freud had become a "minister" after all, as the prophet in the Prater had foretold—a minister not in the political sense, but in a medical sense, ministering to his dying father. Not Hannibal the general, but Winckelmann the scientist.

What was Freud's interpretation of the Revolutionary Dream? Strikingly, he ignored the flight from politics which is so prominent in its scenario and manifest content. Instead, he brought his analysis to focus in the final scene on the station platform from which, in his view, the whole Revolutionary Dream received its basic meaning. Freud recalled two episodes of his childhood in which his father had reprimanded him for urinating. In one of them he finally found the personal, childhood source of his "pathological ambition," before it had been translated in his youth into the political sphere and in his maturity to the professional sphere. In that episode little Sigmund had "disregarded the rules which modesty lays down" and urinated in his parents' presence. His insensitive father had reprimanded him with a prophecy that rang like a curse: "This boy will come to nothing." In the final scene of the Revolutionary Dream, the grown-up Dr. Freud reversed this situation. Instead of the strong father reprimanding the weak son for urinating, the strong son helped the weak father in urinating. "As though I wanted to say," Freud comments, "'You see I *have* come to something.'" Vengeance, of an intellectual kind, is being taken here not on Rome and not on Count Thun, but on the father. As the father replaces the prime minister on the station platform, patricide replaces politics.

Is not this something more as well: revenge on politics itself? Freud suggested this explicitly in a footnote where he connected his victory over his father with his victory over politics:

> the whole rebellious content of the dream, with its *lèse majesté* and its derision of the higher authorities, went back to rebellion against my father. A Prince is known as father of his country; the father is the oldest, first, and for children the only authority, and from his autocratic power the other social authorities have developed in the history of human civilization.[39]

In this passage Freud adumbrated his mature political theory, the central principle of which is that all politics is reducible to the primal conflict between father and son.[40] The Revolutionary Dream, miraculously, contained this conclusion in its very scenario: from political encounter, through flight into academia, to the conquest of the father who has replaced Count Thun. Patricide replaces regicide; psychoanalysis overcomes history. Politics is neutralized by a counterpolitical psychology.

*Wissen macht frei*—so ran the great slogan of Austrian liberalism.

[39] *Ibid.*, 4: 217 n.1.
[40] This theory was set forth in Freud, *Totem and Taboo* (1913), *SE*, 13.

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Freud had not paid his debt to his father as a revolutionary doctor like Adolf Fischhof in 1848 or Victor Adler in 1898. Instead Freud would pay it as a scientific subversive. He dissolved the oath of Hannibal by his counterpolitical discovery: the primacy of infantile experience in the determination of human behavior. With that discovery, a new road to Rome lay open.

BEFORE FREUD COULD go to Rome he had to fulfill two more tasks left over from the victory over politics and father in the Revolutionary Dream. He had to send his father's ghost to an appropriate kind of Valhalla, and he had to universalize his personal experience as a scientific finding. He solved the first task with a dream of Hungary, the second with a myth of Thebes.

The Dream of Hungary must have been dreamt some time after October 1898, when Count Thun was still wrestling with the problem of pulling the two halves of the Empire, the Austrian and the Hungarian, together. After both governments had agreed on the Ischl formula, the fruit of Count Thun's negotiations, the Hungarian nationalists rebelled. Resorting to obstructionism in their parliament, in emulation of the Germans in the Reichsrat, in February the Hungarian nationalists brought down the government that had initialed the agreement.[41] It was in this situation that Freud in his dreams assigned his father to the important role of peacemaker. Freud reported his dream as follows:

> After his death my father played a political part among the Magyars and brought them together politically. Here I saw a small and indistinct picture: a crowd of men as though they were in the Reichstag; someone standing on one or two chairs, with other people around him. I remembered how like Garibaldi he looked on his deathbed, and felt glad that that promise had come true.[42]

A promise come true indeed! Here was Freud's father synthesizing in his person the two traditional allies of Austro-German liberals against the Habsburgs: Italian and Hungarian nationalists. As Garibaldi, father Freud was a modern Hannibal, a populist political-military hero who also failed to take Rome (from the pope in 1867). As Hungarian leader, Jakob Freud was in his turn stepping into the minister's shoes and solving the Hungarian problem, which would soon bring down Count Thun.

As parliamentary ruler, reconciler of the irreconcilable Hungarians, father Jakob Freud thus made good his failures of Sigmund's youth. Freud found in the dream, along with elements denigrating his father, the wish that the father might stand "great and unsullied" before his children after

[41] Ervin Pamlenyi, ed., *Die Geschichte Ungarns* (Budapest, 1971), 450–54. The Ischl clauses were agreed upon on August 30, 1898. The Hungarian opposition inaugurated in October 1898 the obstruction crisis which provided the context of Freud's dream. Grinstein, *Freud's Dreams*, 376, suggests a later date than the crisis itself would in my view require.

[42] Freud, *The Interpretation*, *SE*, 5: 427–28.

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his death. Freud makes no comment on the fact that the paternal apotheosis should be political. Yet the substance of the dream speaks clearly enough: a successful Father Garibaldi-Freud in Hungary made his son's pursuit of politics unnecessary and cancelled the debt of 1868.

Freud's second task in opening his road to Rome was professional: to draw theoretical consequences from the experience of the patricidal impulse he had found in the Revolutionary Dream. This he did by identifying a mythic archetype, the myth of Oedipus, to give form to his finding that "the death-wish against parents dates back to earliest childhood."[43] He appropriated the Oedipus myth in such a fashion as to bring out the *sexual* dimensions it contains. In so doing he pushed the interpretation of dreams as a whole one step further down from personal infantile experience, to which he had traced, in order to expose, his political encounters, to the childhood of the human race. The mythic layer is the deepest in *The Interpretation of Dreams,* where the individual experience of the unconscious is found embedded in the universal archetypal experience of primitive man. Here personal history joins the ahistorical collective.

We cannot here treat the significance of the Oedipus legend for Freud's thought or for the structure of *The Interpretation of Dreams.* Let us only suggest a peculiarity of Freud's treatment of Oedipus related to the problem of neutralizing politics. Freud pays no attention to the fact that Oedipus was a king. As for Nietzsche and other modern philosophers so for Freud the Oedipus quest was a moral and intellectual one: to escape a fate and acquire self-knowledge. Not so for the Greeks. Sophocles' *Oedipus Rex* is unthinkable except as *res publica,* with its regal hero motivated by political obligation: to remove the plague from Thebes. Though Oedipus's guilt is personal, his quest to discover it and his self-punishment are a public matter and restore public order. Freud's Oedipus is not *Rex,* but a thinker searching for his identity and its meaning. By resolving politics into personal psychological categories, he restores personal order, but not public order. Dr. Freud left Thebes languishing still under the plague of politics, while he elevated his slain father's ghost to kingship in the Dream of Hungary.

Was there then nothing left of Hannibal-Freud, nothing of Figaro-Freud, nothing of Freud the challenger of the count in the Revolutionary Dream? The Latin legend on the title page suggests an answer: *Flectere si nequeo superos, Acheronta movebo* (If I cannot bend the higher powers, I shall stir up hell [the river Acheron]). These words from Vergil's *Aeneid* are spoken by Juno, divine defender of Semitic Dido against Aeneas, founder of Rome. Having failed to persuade Jupiter to let Aeneas marry Dido ("to bend the higher powers"), Juno summons from Hell a fury, Allecto, who unleashes seething passions of sex and military aggression in the

[43] *Ibid.,* 4: 257.

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camp of Aeneas's allies. Vergil paints a fearsome portrait of Allecto—a gorgonlike phallic female "alive with black and writhing snakes," a bisexual monster.[44] Freud cites Juno's words again in his text in an important place, where he wishes to point up the overall significance of his research into dreams. After repeating the quotation, he says, "The interpretation of dreams is the royal road to a knowledge of the unconscious activities of the mind." And in a footnote he adds: "This line [the legend] is intended to picture the efforts of the repressed instinctual impulses."[45]

Freud was not the first to use Juno's threat to stir up hell as a motto for a work with subversive implications. Again the trail leads us back to politics, this time to the socialist Ferdinand Lassalle.[46] One of Lassalle's most brilliant pamphlets, *The Italian War and the Task of Prussia* (1859), also carried the words *Flectere si nequeo, superos Acheronta movebo* on its title page. On July 17, 1899, Freud wrote to Fliess that he had chosen this as the motto for *The Interpretation of Dreams;* in the same letter, but making no connection, he mentioned that he was taking "the Lassalle" on vacation for summer reading.[47] Conversant as he was with the *Aeneid,* Freud needed no Lassalle to discover the lines that graced his title page.[48] Yet the strong correspondence between Lassalle's pamphlet and Freud's cathexis of youthful political predilections and current political anxieties in the nineties makes it highly likely that Freud read it. *The Italian War* contained many themes and attitudes that we have found in *The Interpretation of Dreams:* the hatred of Catholic Rome and the Habsburgs as the bastions of reaction; the linkage of Garibaldi and the Hungarians as liberal protagonists; and, like Freud in his dream-confrontation with Count Thun, the espousal of German national feeling against the aristocratic Austrian.[49] In intellectual strategy lay a further affinity. Lassalle, too, played with repressed forces, in his case the revolutionary forces of the people. That is why he chose the Vergil motto for his pamphlet. In it, Lassalle tried, à la Juno, to persuade the "higher powers" of Prussia to lead the German people, in alliance with the Italians, in a war of national unification against the Habsburg state. But behind his persuasion lay a threat: Should Prussia fail to act, her rulers would learn to their sorrow "in what strata of opinion power [actually] resides." Lassalle thus

[44] Vergil, *The Aeneid,* bk. 7, lines 286–571, especially 312, 323–29, 445–55.

[45] Freud, *The Interpretation, SE,* 5: 608.

[46] This discovery was made by Ernst Simon, "Sigmund Freud the Jew," Leo Baeck Institute, *Year Book,* 2 (1957): 301.

[47] Freud to Fliess, July 17, 1899, in Freud, *Origins,* 286. Freud does not mention any specific title in his letter, only "den 'Lasalle'" [*sic*]. While Lassalle's separate works were not readily available at the time, several collected editions which would include "The Italian War . . ." were issued in the nineties. One of these, Erich Blum's *Ferdinand Lassalles politische Reden und Schriften* (Leipzig), appeared in 1899, when Freud was polishing the manuscript of *The Interpretation of Dreams.*

[48] He first cited them in a letter to Fliess on December 4, 1896. Freud, *Origins,* 172.

[49] Ferdinand Lassalle, *Gesammelte Reden und Schriften,* ed. Eduard Bernstein (Berlin, 1919), 1: especially 16–17.

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threatened "those above" with the latent forces of national revolution, with stirring up a political Acheron.[50] Freud would have found it easy to appropriate Lassalle's legend, transferring the hint of subversion through the return of the repressed from the realm of politics to that of the psyche.

The contents of Freud's dreams often confirm relationships that other evidence only suggests. In a dream of Lassalle,[51] Freud virtually celebrated the primacy of psychoanalysis over politics that his use of Lassalle's motto implies. Along with another Jewish German political leader, Eduard Lasker, Lassalle serves the dreaming Freud as a symbol of the fatal power of sex. Characteristically, Freud ignores in his interpretation of this dream the fact that the material carriers of the message were both politicians. He assigns them significance only as "men of talent." Both "came to grief over a woman," and thus illustrated the damage—in Lasker's case organic, in Lassalle's neurotic—"caused by sexuality." Freud found in the dream a monitory confirmation of his own fears, as another "man of talent," of coming to grief over a woman. In the dream Freud also conquers the power of his own sexual temptation by his clinical understanding of neurosis, while the two Jewish politicians are undone by it. Sex is stronger than politics, the dream seems to us to say, but science can control sex.

Freud's Acheron of "repressed instinctual impulses," like Lassalle's Acheron of the angry *Volk*, certainly had subversive implications for those in political authority. In the final pages of *The Interpretation of Dreams*, Freud took pains to assuage the fears such findings might arouse. Again he chose Roman lore for his example:

> I think that the Roman Emperor was in the wrong when he had one of his subjects executed because he had dreamt of murdering the emperor. . . . would it not be right to bear in mind Plato's dictum that the virtuous man is content to *dream* what a wicked man really does? I think it best, therefore, to acquit dreams.[52]

Freud had earned the right to these lines, with their comforting message to "those above," whom Juno's threat might alarm. Having exhumed his own political past through dream analysis, he had overcome it by identifying his political obligations and impulses with his father, explaining them away as attributes of his father's ghost.

AND SO THE SPELL of Hannibal's oath was broken. His theoretical work and his self-analysis accomplished in *The Interpretation of Dreams*, Freud actually entered the Eternal City in 1901, nearly five years after

[50] *Ibid.*, 1: 112. See his frank discussion of his political strategy in a letter to Marx, n.d. [mid-May], 1859, in Franz Mehring, ed., *Aus dem literarischen Nachlass von Karl Marx, Friedrich Engels und Ferdinand Lassalle* (Stuttgart, 1902), 4: 150.

[51] The dream is called "Autodidasker," after the single word that was its manifest content. Freud, *The Interpretation, SE*, 4: 298–302.

[52] *Ibid.*, 5: 620.

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his father's death, not "to take vengeance on the Romans," but as intellectual pilgrim and psychoarcheologist, in the footsteps of Winckelmann. He wrote: "It was an overwhelming experience for me, and, as you know, the fulfillment of a long-cherished wish. It was [also] slightly disappointing." Freud described his varied reactions to three Romes: the third, modern, was "hopeful and likeable"; the second, Catholic Rome, with its "lie of salvation" was "disturbing," making him "incapable of putting out of my mind my own misery and all the other misery which I know to exist." Only the Rome of antiquity moved him to deep enthusiasm: "I could have worshipped the humble and mutilated remnant of the Temple of Minerva."[53]

Did Freud think of the significance of his spontaneous urge to worship Minerva in her ruins? Like Juno's hellish fury, Allecto, whom Freud had invoked on his title page, Minerva, too, was a bisexual goddess. But where Juno's phallic female unleashed terror against the founders of the city, the virgin goddess was *protectrix* of the civic order, using her spear, her snaky aegis and her gorgon-studded shield to repel the enemies of the polis. Unlike Allecto, Minerva was a brain-child, sprung in a virgin birth out of the head of her father.[54] Her statue stood, and still stands, before the parliament building in Vienna, the symbol of liberalism's faith in a rational polity. Minerva's wisdom was of a special kind that reconciled one to Jupiter, to the structure of necessity and the reality of power.

In the letter with which this essay began, Freud had wryly celebrated his promotion to a professorship in the caricature of a political triumph. We can now see his humor as somewhat more bitter than its surface would suggest. Promotion came as a personal and professional triumph, but at a high moral cost.

> It was my own doing in fact. When I got back from Rome, my zest for life had somewhat grown, my zest for martyrdom diminished. . . . So I made up my mind to break with my strict scruples and take appropriate steps. . . . One must look somewhere for one's salvation, and the salvation I chose was the title of professor.[55]

The brilliant, lonely, painful discovery of psychoanalysis, which made it possible for Freud to overcome his Rome neurosis, to kneel at Minerva's temple, was a counterpolitical triumph of the first magnitude. By reducing his own political past and present to an epiphenomenal status in relation to the primal conflict between father and son, Freud gave his fellow liberals an ahistorical theory of man that could make bearable a political world spun out of orbit and beyond control.

[53] Freud to Fliess, Sept. 19, 1901, in Freud, *Origins*, 335–36.
[54] Norman O. Brown, *Love's Body* (New York, 1966), 77–78.
[55] Freud to Fliess, Mar. 11, 1902, in Freud, *Origins*, 342.

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# Tempest in Clio's Teapot: The American Historical Association Rebellion of 1915

RAY ALLEN BILLINGTON

TWICE IN ITS THREE-QUARTERS of a century the American Historical Association has been plunged into turmoil by attempts of a dissatisfied minority to wrest control from an elitist group in power. Each attempt has been rejected by the majority of the members, but each has encouraged overdue reforms generally tending toward more democratic procedures. So it was in recent years when an onslaught from the Left led to a more representative executive council and a transfer of basic authority from the business meeting to the membership as a whole. And so it was in 1915 when a small band of insurgents stirred a teapot tempest that ended with a shift in power from the executive council to the business meeting.

The insurgents of 1915 had cause to complain, for the association at that time was in the hands of a self-perpetuating group of eminent professors from large universities who had, unknowingly, developed an "Establishment" philosophy and an "Establishment" system of management. They operated through an interlocking system of directorships; all officers were chosen by a nominating committee that was itself selected by the executive council; the second vice-president ascended automatically to the presidency and then to lifetime membership on the council; the secretary and treasurer held office virtually for life; the six-man board of editors of the *American Historical Review* suggested its own replacements, which were automatically confirmed by the council. In effect the 2,800 members of the association were governed by two or three dozen professors from a half-dozen universities, drawn largely from those in their senior years, for the ex-presidents who served for life on the council could outvote the six elected members. Within this governing clique an elite inner core known as the "Nucleus Club" met annually at a gay champagne dinner where fundamental policy decisions were made—or so the mass of the members believed. Those in the Nucleus Club saw nothing wrong; in their own eyes they were self-sacrificing servants of the profession, giving generously of themselves to provide historians with leadership and guidance. A good many of the rank and file were less sure and needed



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only the right intellectual climate and a dedicated leader to raise the standard of revolt.

The proper climate was provided by Progressivism; insurgency was in the air during the second decade of the twentieth century, and all trusts or interlocking directorships, even those operated by staid historians, were fair game for crusaders. The success of the leaders of the AHA insurgents is less understandable, for the three primarily responsible for the rebellion seemed not of the stuff of which heroes are made. Dunbar Rowland, director of Mississippi's Department of Archives and History, was an overambitious self-seeker with an inordinate appetite for posts he lacked the talent to hold; he had campaigned openly for a dozen offices, ranging from membership on the executive council to assistant secretary of state, and he believed that all had been denied him by the "Eastern Establishment" because he was a Southerner. Frederic Bancroft nursed a more personal grievance. He lived modestly well in Washington as librarian of the Department of State, where he had ample time to pursue his chosen vocations as gentleman scholar and gadfly of the historical profession, but he was always under the shadow of his brother Edgar A. Bancroft, a highly successful attorney for the International Harvester Company. Excitable, irascible, subject to violent temper outbursts, Frederic Bancroft could at times be a highly difficult person. The third of the triumvirate, John H. Latané, professor of history at Johns Hopkins, played a lesser role, but he was always ready to join his fellow plotters to voice his prejudices as a Southern victim of the Northeastern historians who dominated the profession.

These three were the ringleaders, but they gathered about themselves a handful of conspirators who played their parts and then left the stage. It is difficult to identify most of them, for Frederic Bancroft was inclined to count as followers a number of younger men who favored reform but opposed his tactics; Ulrich B. Phillips, William E. Dodd, John Vincent, and perhaps Eugene C. Barker were of this group. On the other hand Bancroft was as quick to dismiss a convert as he was to embrace one, should that individual have the audacity to disobey his orders; some whom he castigated were as eager for radical change as he. To count noses under these conditions is impossible, but Charles H. Ambler and Edward McMahon were consistently in the Bancroft camp, together with a half-dozen lesser lights from the South and West. The rebels were few in numbers, but they were a noisy crew and cost the Establishment a good many sleepless nights before their cause was lost.

Their purposes were laudable: they hoped to democratize the government of the association by making the executive council truly representative and by shifting power to the business meeting, and they wanted to secure ownership of the *American Historical Review* for the association. That excellent journal, founded in 1895 by twenty-six "guarantors" who

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underwrote its expenses, had been since that time the legal property of the board of editors, who supplied members of the association with copies at two dollars a head. Few would quarrel with these two objectives, for democratic reforms were overdue and the association was now strong enough to assume the financial burdens of the *Review*. Where the insurgents and the mass of the members parted company was on the means needed to secure these changes. Most of the membership trusted the existing Establishment and counted on them to respond to the popular will. Bancroft, on the other hand, was convinced that these entrenched bureaucrats were so bent on self-seeking that they must be ousted before reform was possible. Two particularly must go: J. Franklin Jameson, editor of the *Review* and perennial secretary of the association, and Frederick Jackson Turner, Jameson's right-hand man, who was completing his second six-year term on the board of editors and who, as an ex-president, was a lifetime member of the executive council. Bancroft's irrational campaign to drive these two men, and some of their colleagues, from power underlay the Rebellion of 1915.

THE OPENING GUN was fired two years earlier, and by Dunbar Rowland rather than Frederic Bancroft. Hurt by a friend's refusal to support him when he suggested himself for the executive council, Rowland took violent exception when the usual motion was made by the nominating committee at the business meeting in 1913, asking for the usual unanimous ballot to elect its candidates. He was on his feet at once. The whole system was undemocratic, and a self-perpetuating "ring" was leading the association into oligarchy. Officers should be elected by a ballot sent to all members. The brief discussion that followed was amicable enough, and so was a resolution urging the nominating committee to investigate an improvement in election procedures. "It was," Jameson wrote, "a rather interesting tempest in a teapot, but didn't blow off the lid."[1] Nor did Rowland's next moves. Incensed by his rebuff, he hurried a letter to the *Nation* charging that the association was "oligarchical" and demanding a general uprising against the "ring." The result was hardly encouraging: a spate of letters defending the existing system and praising the self-sacrificing contributions of the Establishment. "I do not," wrote one historian, "conceive of the Council as an oligarchy grasping for power, but rather as a hard-working body having increased labors thrust upon it."[2] Rowland's

[1] J. Franklin Jameson to Waldo G. Leland, Jan. 8, 1914, in Elizabeth Donnan and Leo F. Stock, eds., *An Historian's World: Selections from the Correspondence of John Franklin Jameson* (Philadelphia, 1956), 166. The business meeting is described in Jameson, "The Meeting of the American Historical Association at Charleston and Columbia," *AHR*, 19 (1914–15): 467–93.

[2] *Nation*, Jan. 22, 1914, p. 82; Jan. 29, 1914, pp. 9, 107; Feb. 5, 1914, pp. 13–14, 132; Mar. 19, 1914, p. 297. The historian who defended the council was Sidney B. Fay. Rowland reprinted these and other letters, together with his own extensive comments, in a pamphlet, *The Government of the American Historical Association: A Plea for a Return to the Constitution* ([Washington, 1915]), 7–13.

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stout reply struck not a spark. More effective leadership was needed to launch a revolution.

The man destined to assume the mantle seemed at this time hopelessly lost to the Establishment. Frederic Bancroft, newly elected to the executive council, viewed Rowland's campaign with tolerant amusement; perhaps offices had been monopolized by a small group, but they were dedicated men willing to sacrifice themselves for the profession. "Since I have come to associate more closely with the 'oligarchy,'" he told a friend, "I find that they are the mildest and most generous of despots."[3] Rowland's plan for a mail ballot would never work. Better leave matters as they were.

So it was that Bancroft attended his first meeting of the executive council in New York in November 1914, brimming with friendliness and ready to enjoy himself thoroughly. He dined handsomely on the Friday evening after Thanksgiving at the expense of the retiring president, Andrew C. McLaughlin, as was the custom (with two of the wealthier ex-presidents as usual footing part of the bill), heard the usual endless committee reports, lunched well at the Metropolitan Club as a guest of the association, and voted with the majority on two important issues: to name a committee of nine to study the constitution with an eye to democratic improvements, and to request that committee to investigate relationships between the association and the *Review*.[4] Bancroft even promised that his brother Edgar would supply free legal advice on the *Review*'s ownership if Jameson would provide him needed information. "I had a thoroughly good time in New York," Frederic Bancroft told his brother, "and met the men I like best. They are able, fine and interesting."[5] All was sweetness and light when the council adjourned, to meet again at the annual convention in Chicago a month later.

Oddly enough, Edgar Bancroft's opinion started his brother singing a different tune. Frederic Bancroft had, from the time of his election to the executive council, flirted with the idea of finding a cause that would heighten his prestige; "I should," he confessed to William E. Dodd, "like to experience the sensation of being a progressive and a reformer."[6] Here was his chance, for Edgar Bancroft had the audacity to conclude that the *American Historical Review* was legally the property of the board of editors, who could do with it as they liked so long as it was used "for the cause

[3] Frederic Bancroft to James A. Woodburn, June 24, 1914, Frederic Bancroft Papers, Department of Special Collections, Columbia University Library, box 1. For similar views see Bancroft to Lyman Chalkley, Jan. 6, 1914, and Bancroft to Dunbar Rowland, Feb. 3, Mar. 5, 1914, *ibid.*

[4] The manuscript minutes of this meeting are in the American Historical Association Papers, Division of Manuscripts, Library of Congress, box 256, folder "Minutes of Executive Council, 1909–1915." Rough minutes kept by Turner of the meeting of the board of editors held in New York at the same time are in the Frederick Jackson Turner Papers, Henry E. Huntington Library and Art Gallery, TU box 56.

[5] Bancroft to Edgar A. Bancroft, Nov. 30, 1914, Bancroft Papers, box 2. On Dec. 3 Frederic Bancroft wrote his brother again, asking him to investigate the legality of the *Review*'s ownership. *Ibid.*

[6] Frederic Bancroft to William E. Dodd, Mar. 23, 1914, *ibid.*, box 1.

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of American history, its writers, students, and readers."[7] Frederic Bancroft bristled when he read his brother's letter. Edgar might be technically correct, but the arrangement cried for reform. Either the *Review* must be transferred to the association or a new magazine founded, one that would omit the nitpicking details pleasing to the "dry-as-dust" professors in the "academic trust." Transfer was preferable, but given Bancroft's prejudices, this could only be accomplished by breaking up the editorial "ring" in control, for the "ring" would never relinquish such a valuable property voluntarily. Bancroft was not ready to begin a frontal assault on Jameson, but the two members of the board of editors scheduled to retire that year must be replaced by Bancroft's own men.

Bancroft was ready to pounce when the executive council gathered in Chicago that December. As usual one of the first orders of business was naming new members to the editorial board. One of the two scheduled to be replaced had been persuaded to accept another term, but Andrew C. McLaughlin of the University of Chicago was retiring, and the board recommended that he be succeeded by his colleague, William E. Dodd. This suggestion was anathema to Bancroft; to replace one Chicago professor by another would perpetuate Establishment control and imply that the university was entitled to a permanent place on the board. Why not name instead William Roscoe Thayer, a proper Bostonian and gentleman scholar who at least knew how to write? The council backed down before Bancroft's vehemence but took the easy way out by referring the question back to a committee, which eventually named a compromise candidate, Carl Becker of Cornell, to the post.[8]

Frederic Bancroft had won his first victory; when he met Dunbar Rowland in the corridor a few minutes after the meeting adjourned he announced that the "ring" had surrendered and that the insurgents could rest in peace. Not Rowland. He had launched the reform movement and was determined to stay in the spotlight. His chance came when the nominating committee reported at the annual business meeting. It had, the committee reported, sought the advice of the entire membership on election procedures, but only 222 had bothered to reply, and of these seventy-three per cent favored the existing system. Hence the committee proposed a slate of officers with Henry Morse Stephens ascending to the presidency, George Lincoln Burr to the vice-presidency, and Worthington C. Ford recommended for the second vice-presidency. When the chairman moved a unanimous vote for their election, Rowland leaped to his feet. In what even Bancroft described as a "flapdoodle speech," Rowland demanded that the business meeting vote by secret ballot. When a secret

[7] Edgar A. Bancroft to Jameson, Dec. 24, 1914, AHA Papers, box 370.

[8] This meeting of the executive council is described in Bancroft to William A. Dunning, Jan. 6, 1915, Bancroft Papers, box 2. Full minutes of the meeting, in several versions, all in Turner's hand, are in Turner Papers, TU box 56.

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ballot was taken Stephens received 129 votes for president, Dunbar Rowland one.[9]

Rowland's days as a reformer were numbered, but this he refused to accept. When the business meeting turned to authorizing the committee of nine to propose democratic changes in the association's government, his various motions either died for lack of a second or were overwhelmingly rejected. In the end the meeting agreed to grant the committee of nine authority to investigate the entire structure of the association as well as its relationship with the *Review*. Rowland was tossed a crumb by appointment to the group of five selected to choose the committee, but again he was consistently outvoted. As ultimately constituted, the committee was fairly representative of the membership, but with Andrew C. McLaughlin as its chairman.[10] The choice of McLaughlin was particularly distasteful to Bancroft and his ilk; as a former editor of the *Review,* McLaughlin in their eyes personified the Establishment. Yet orderly reform from within seemed triumphant as the association prepared to wash its own linen in private. "I do not think we need to be on the lookout for any trouble," Jameson wrote a friend.[11] He was, as time soon showed, a thoroughly inaccurate prophet.

FREDERIC BANCROFT WAS RESPONSIBLE. All that he saw and heard at the Chicago meeting convinced him that the revolution would succeed under more capable leadership and that he should take command. This meant recruiting an army. That winter of 1914–15 his letters circulated widely through the South and West, reciting the evils bred of "ring" domination, stirring sectional prejudices by hinting at an "Eastern Establishment," and urging readers to "help some of us conservative reformers improve conditions so that those outside a small circle of men and universities, can have a fair show."[12] The letters struck a responsive chord, even among levelheaded historians. Clarence W. Alvord, then a young professor at the University of Illinois, mirrored the sentiments of many when he sought Frederick Jackson Turner's advice on how to reply. The *American Historical Review,* Alvord complained, seemed to discriminate against articles on the West and South; he and his friends were shifting their loyalty to the *Mississippi Valley Historical Review* and its parent association. Unless the American Historical Association mended its ways by

[9] Bancroft to Dunning, Jan. 6, 1915, Bancroft Papers, box 2. Full manuscript minutes of the business meeting are in AHA Papers, box 256, folder "Minutes of Council Meetings, 1908–1915."

[10] The committee of nine, as finally constituted, consisted of Ephriam D. Adams, R. D. W. Conner, Isaac J. Cox, William A. Dunning, Max Farrand, Andrew C. McLaughlin, W. T. Root, James Ford Rhodes, and James Sullivan, a Boston high-school teacher.

[11] Jameson to Clarence W. Bowen, Jan. 12, 1915, AHA Papers, box 27, folder "Bowen."

[12] Typical are Bancroft to Matthew P. Andrews, Jan. 6, Feb. 11, 1915; to J. L. Conger, Mar. 24, 30, 1915; to Edward McMahon, Mar. 6, 1915; to R. M. McElroy, Mar. 8, 13, 1915; and to Dodd, Mar. 10, 1915, Bancroft Papers, box 3.

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giving equal time with Easterners and Europeanists, other regional societies would be formed and a central historical agency cease to exist. Turner passed these complaints along to Jameson, who replied heatedly that the superiority of articles in the European field explained their numbers, not prejudice against Midwestern Americanists.[13] But the underlying dissatisfaction was still there. From that time on Turner realized that reform was necessary, even though he had little sympathy for Bancroft's techniques.

Turner's conversion brought the division within the association into clear focus. At one extreme were a few of the old guard who opposed change; Jameson was inclined to this view occasionally, although he generally favored moderate reform. At the other extreme were the insurgents under Frederic Bancroft who were convinced that the committee of nine was only a tool of the inner "ring" governing the association and would make no genuine concessions. The insurgents had a definite platform: less power for ex-presidents, a more representative executive council, ownership of the *Review,* and an end to the interlocking relationship between its board of editors and the council. Between these extremist groups were the bulk of the members, with Frederick Jackson Turner their most vocal spokesman. These favored the changes advocated by the insurgents, but believed that the committee of nine could be counted on to achieve them.

If at this point Bancroft had sought an alliance with Turner and the moderates he would have won the gratitude and respect that his starved ego sought. Instead he shifted his attack from issues to personalities. The "inner ring," he believed, were so greedy that they would never voluntarily surrender their authority; hence they must be scourged from the association before reform could succeed, particularly Jameson from the editorship of the *Review* and George Lincoln Burr from his scheduled presidency of the association. For Burr, Bancroft had an abundance of scorn. This "feeble, shilly-shallying half-cry" of a man was more a librarian than a professor, with only two books and a handful of graduate students to his credit; furthermore, if elevated to the presidency he would be only another "Jameson poodle" doing the will of the "ring."[14] Burr must be toppled along with Jameson, Turner, and the rest of the Establishment. But how?

Bancroft's plan of attack was suggested by memories of the executive council's luncheon at the Metropolitan Club that he had enjoyed the preceding November. That lunch, he had learned when the council met

[13] Clarence W. Alvord to Turner, Jan. 22, 1915, Turner Papers, TU box 23; Turner to Alvord, Jan. 27, 1915, Clarence W. Alvord Papers, Illinois Historical Survey of the University of Illinois, Urbana; Turner to Jameson, Feb. 5, 1915, AHA Papers, box 283, folder "T"; Jameson to Turner, Feb. 9, 10, 1915, Turner Papers, TU box 23.

[14] Bancroft to Ruth Putnam, Feb. 13, 1915; to Edgar A. Bancroft, Mar. 15, 1915, Bancroft Papers, box 3.

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in December, had cost no less than $3.25 a plate, with the association paying a bill of $77. This was a gross extravagance at a time when the association operated on a deficit and might be forced to raise dues from $3 to $5. One wrong could be righted at once; before that meeting was over Bancroft had joined with Archibald C. Coolidge in presenting a check for $77 to the association, announcing at the same time that he would collect $3.25 from each member (all eventually paid, save John B. McMaster).[15] Now, in the spring of 1915, as Bancroft thought back over that triumph, he began to wonder. Had the "inner circle" been squandering the association's limited funds on lavish living? Had they used their expense accounts to bilk the underpaid scholars who supported them? An investigation might embarrass the "ring" and even produce evidence that would oust the whole lot.

This meant examining the books of the association and the *Review*. Those of the former were opened without question to a council member, but the board of editors had less reason to share their secrets with an outsider. Moreover Bancroft's peremptory demands were annoyingly phrased; he insisted on "a full and explicit statement as to all payments that have been made to any member of the Editorial Board, on any account whatever, during the last three years."[16] Better to keep him quiet, however, than to stand on principle. Hence the board's treasurer, George Lincoln Burr, returned the current account book to Jameson, who let Bancroft know that all were available in his Washington office. Bancroft could drop in at any time. "There is," Jameson went on, "generally a cop at the corner nearby. Still, you might want to slip a pair of handcuffs into your side pocket before coming over." Such a jocular tone was understandable. Surely no one—not even Frederic Bancroft—could believe that his fellow historians were guilty of malfeasance. "Leland and I," Jameson wrote Turner (referring to Waldo G. Leland, his assistant), "mean to stand by and 'josh' him while he does it."[17]

Jameson's awakening was as sudden as it was rude. Bancroft informed him that he was no accountant and had no desire to spend hours examining the books of the board and council. Bancroft now wanted what he had requested in the first place: a list of all expenditures for the past three years. Burr and Jameson thought best to comply, even though they must have seen in Bancroft's irrational outburst a sign of what was to come. Well they might, for Bancroft magnified this trivial exchange out of all perspective. Burr and Jameson were insidious plotters; they had "hustled" the account books off to Washington where they could be guarded by

[15] Bancroft to Woodburn, Dec. 18, 1914; to Bowen, Dec. 21, 1914; to Dunning, Jan. 6, 1915; to Archibald C. Coolidge, Jan. 6, 1915, *ibid.*

[16] Bancroft to George L. Burr, Mar. 8, 1915, George Lincoln Burr Papers, Division of Regional History and Archives, Cornell University Library, Ithaca, New York.

[17] Jameson to Bancroft, Mar. 17, 1915, Bancroft Papers, box 3; Jameson to Turner, Mar. 13, 1915, Turner Papers, TU box 24.

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Jameson rather than the weaker Burr; the books could be seen only by appointment, which meant that Jameson would know when Bancroft was coming and could spirit them into hiding; the whole business was designed to prevent him from copying the damning evidence that would disclose the "ring's" illegal maneuvers to the membership.[18]

Given these prejudices, Bancroft must have been thoroughly disappointed as he examined the accounts. The association had been charged for railroad fare to the council meetings and the *Review* for travel to meetings of its board of editors, but that was all. Bancroft pretended to be shocked by the amount of some of the charges—$16.25 for Charles Homer Haskins to journey between Boston and Washington, $62.70 for James Woodburn to make the trip from Indianapolis, $3.25 paid Turner for writing a book review—but there was no misuse of funds. Or was there? Just to make sure Bancroft wasted the time of a good many railroad clerks by asking the exact fare between Indianapolis and New York, Cambridge and Washington, and dozens more, only to find that the figures corresponded to the penny with those charged.[19] His hopes of proving dishonesty were dashed.

He could still charge gross extravagance and did so with a vengeance. Why should the council and board live in luxurious hotels and dine at expensive clubs? Professors were unaccustomed to such posh entertainment. "Although you and I may like a rich man's club," he confided to a friend, "I doubt if it is wise for the historical brethren as a rule to cultivate luxurious tastes."[20] Why should the board of editors meet in December when they had met only a month before? Obviously this was a nefarious device to force the *Review* to pay their travel to the annual meeting. Why should the council members collect expenses for attending the yearly meetings of the association? Why should presidents be compensated when they came to the meeting to give their presidential addresses? Turner had collected $54 to hear himself talk at Indianapolis, Albert B. Hart $7.50 to listen to himself in New York, Charles Francis Adams $21.65 for the privilege in Washington. McLaughlin had milked the association of no less than $1,716.34 during his years of committee work.[21]

These revelations were hardly earth shattering, but they conjured up a series of new questions in Bancroft's mind, and with them a new barrage of demands. Had the executive council a legal right to charge its luncheons to the association? Was it authorized to pay expenses for committee chairmen who reported at its meetings? What were the exact funds paid

[18] Bancroft to McMahon, Apr. 1, 1915; to Worthington C. Ford, Apr. 4, 1915; to Ulrich B. Phillips, Apr. 1, 1915; to McElroy, Mar. 30, 1915, Bancroft Papers, box 3; Bancroft to Bowen, Apr. 5, 1915, AHA Papers, box 370.

[19] Bancroft to Passenger Agent, Pennsylvania Road, Apr. 7, 1915; to Chief Clerk, Interstate Commerce Commission, Mar. 30, 1915, Bancroft Papers, box 3.

[20] Bancroft to Bowen, Jan. 19, 1915, AHA Papers, box 370.

[21] Bancroft to Oswald G. Villard, Apr. 6, 1915; to Ford, Apr. 16, 1915; to Dunning, May 26, 1915; to Justin H. Smith, June 11, 1915, Bancroft Papers, box 3.

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board and council members since the beginning of the association? Were the expenses of the program committee paid from general funds or from the special appropriation for the committee? What was the relationship between the *Review* and the Carnegie Institution of Washington, which paid Jameson's salary as director of its historical division? Who made those arrangements and why? The members would be told how their funds were squandered when they gathered for the December business meeting. "This," Bancroft boasted, "will put an end to the hungry nibbling at the treasury by nearly every man who can humbug himself or give an excuse and keep the matter secret."[22]

Bancroft was making himself a supreme nuisance to the officers of the association, but even more to the board of editors of the *Review*. By this time he had concluded that his brother's opinion—that the journal was properly owned by the editors—was based on neither logic nor law; several eminent Washington judges who had heard the Frederic Bancroft version of events agreed with him that the association was the legal owner. Only the full story of the magazine's founding would reveal that he was right. Once more peremptory demands were dispatched to Burr, as treasurer of the board, and to Turner, as secretary. Would they send him at once all documents, resolutions, and correspondence bearing on the early history of the *Review*? They would, but to assemble the documents necessary would take time. Jameson volunteered for the task and spent most of the summer of 1915 compiling a report sent to association members that fall.[23] His findings were exactly those anticipated: the *Review* had been legally entrusted to the board of editors by its guarantors; it had no association with any university; the members of the editorial board were selected for their editorial skills and knowledge of special fields, not because they were connected with any college.

Jameson might draw his conclusions, but Bancroft had other ideas. He was growing apprehensive as spring merged into summer. His principal lieutenants—Latané in Baltimore, Ambler in the Upper South, D. R. Anderson in the Lower South, McMahon in the Pacific Northwest—were still loyal, while Dunbar Rowland was puffing and blowing as usual, but Bancroft was sadly aware that Rowland was more a liability than an asset and that most of his other followers were bush leaguers.[24] Some of the greats who had encouraged him at first were drifting away. Clarence W. Bowen, the treasurer of the association, who had seemed sympathetic to any move that would reduce the annual deficit, was grumbling by May that Bancroft's charges would not hold water. "It would not take me long,"

[22] Bancroft to Bowen, May 5, 1915, AHA Papers, box 370.

[23] Bancroft to Burr, Apr. 29, 1915, Burr Papers; Bancroft to Turner, June 9, 1915, Turner Papers, TU box 25; Turner to Bancroft, June 26, 1915, AHA Papers, box 283, folder "T"; Jameson to Turner, June 1, 1915, Turner Papers, TU box 25. The vast number of documents copied by Jameson while compiling his report are in the AHA Papers, box 370.

[24] Bancroft to D. R. Anderson, May 18, 1915; to McMahon, June 7, 1915, Bancroft Papers, box 3.

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Bowen assured a friend, "to answer any such criticism at any annual meeting."[25] William A. Dunning, Columbia's distinguished historian, made an even more abrupt about-face. "I think," he told Bancroft, "that you have suddenly been deprived by some malign influence of all your power to form a just or charitable or even rational judgment about certain persons and certain things."[26] Bancroft's army was threatening to melt away before he could make his exposures at the December meeting. Better to reveal the truth now.

He was already in touch with Oswald Garrison Villard, the editor of the *Nation*, who seemed so eager to publish an exposé of the association that Bancroft listed him among the "reformers." The insurgents were promised spicy reading when the article appeared—such a merciless "flaying of hypocrites and self-seekers" that they "would not be able to bite back even if they wished to." The whole nation would be rocked, and Jameson and Leland driven from the profession. "I mean to put the 'dean' and his little man Friday out of business," Bancroft promised.[27] Then the unexpected happened. The *Nation* refused to publish the article! The editors might welcome muckraking exposures, even of historical associations, but they did not welcome the libel suits that Bancroft's extravagant accusations might inspire. Nor were they convinced that washing professional dirty linen in public was the *Nation*'s responsibility. "Petty misuse of the treasurer's funds," he was told, "however wide its bearings, should I feel sure, be discussed within the organization itself." Bancroft had another explanation: Villard had truckled to pressure from Harvard and the university clique that wanted to suppress his evidence. "O shades of Godkin and Garrison!" he wrote.[28]

Bancroft might console himself with such fantasies, but his followers had been told that the article would appear, and they must be satisfied. "We found," they were told, "that we could not rely on the *Nation* for developing the attack." Hence the exposure would be expanded with the help of Rowland and Latané and would be sent as a pamphlet to all three thousand members of the American Historical Association.[29] This time Bancroft delivered what he promised. The sixteen-page *Why the American Historical Association Needs Thorough Reorganization* hardly set the profession on fire when it was distributed early in July 1915. It described Bancroft's heroic efforts to secure information on expense accounts, listed the exorbitant sums spent by the "ring" on travel and entertainment, charged that the editorial board of the *Review* was controlled by six universities (Harvard, Yale, Pennsylvania, Columbia, Cornell,

[25] Bowen to Leland, May 1, 1915, AHA Papers, box 27, folder "Bowen."

[26] Dunning to Bancroft, May 30, 1915, quoted in Jacob E. Cooke, *Frederic Bancroft, Historian* (Norman, 1957), 101.

[27] Bancroft to Edgar A. Bancroft, May 10, 12, 1915, Bancroft Papers, box 3.

[28] Bancroft to H. DeW. Fuller, July 25, 1915, *ibid.* Bancroft quotes Fuller's comment, which was contained in a letter that has apparently not been preserved.

[29] Bancroft to Mayor Conger, June 25, 1915, *ibid.* Almost identical letters were sent that day to Coolidge, Dunning, Ford, McMahon, Phillips, and W. R. Taylor, all in *ibid.*

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and Chicago) and that the board in turn ran the association, and glorified the "reforms" that Bancroft and his fellow insurgents wanted. These could never be secured through the committee of nine, which was rigged to use the "political hocus-pocus" needed to protect the "ring."[30]

When Bancroft predicted that his pamphlet would force the immediate resignation of Jameson and his stooges, he must have realized that he was mouthing absurdities, but he was hardly prepared for what took place. His few followers responded predictably; he was a modern Addison, Swift, and Pope rolled into one, a benefactor of mankind, an unerring marksman who had "fired hot shot into the 'ring'" and sent it scurrying. "I've been watching the papers to see whether its members haven't quietly died, as their only means of escape," wrote one admirer.[31] Those not blinded by Bancroft's blandishments reacted differently. Most were embarrassed rather than horrified by "revelations" that revealed nothing and by unsubstantiated charges against the dignified leaders of the profession. Some among his former supporters dropped from the ranks, while more who had flirted with Bancroft's brand of reform remained discreetly silent. His letters to these backsliders were strangely pathetic. Have you read the pamphlet? How can you sit on the side lines in the face of such overwhelming evidence? "Have you nothing to say in regard to the self-sacrificing professors?"[32] For those who remained silent, pleading soon turned to abuse. "What a hopelessly reactionary old crab you are," one was told. "You are progressing backward into the darkness of the Middle Ages."[33] There was more frustration than truth in such charges.

The wedge that Bancroft was driving between his tiny clique and the many who wanted true reform deepened during the summer. A hysterical letter to the *Nation* beating the dead horse of George Lincoln Burr's elevation to the presidency did little more than inspire effective rebuttals from Albert Bushnell Hart and Edward P. Cheyney, the newest member of the editorial board of the *Review*.[34] A shrill protest levied against the executive council when it balloted its members on a $400 appropriation for the committee of nine led only to another humiliating defeat.[35] Even more humiliating was the reaction to a typical foot-in-mouth performance

[30] Frederic Bancroft, John H. Latané, and Dunbar Rowland, *Why the American Historical Association Needs Thorough Reorganization* (Washington, 1915), 1–16. The quotations are from pages 10–11.

[31] Bancroft summarized these favorable comments and quoted many of them, without naming the authors, in a letter to the *Nation*, Sept. 16, 1915, p. 357. For a typical letter, see Rowland to Bancroft, Aug. 18, 1915, Dunbar Rowland Papers, Mississippi Department of Archives and History, Jackson, Mississippi.

[32] Bancroft to Thomas M. Owen, July 15, 1915, Bancroft Papers, box 3. Similar letters were written to Frank M. Anderson, Smith, W. K. Boyd, Paul Blanchard, Dunning, Yates Snowden, and Charles C. Swisher, all in *ibid.*, boxes 3, 4.

[33] Bancroft to Gaillard Hunt, Aug. 21, 1915, *ibid.*, box 3.

[34] *Nation*, Sept. 16, 1915, pp. 355–57; Sept. 30, 1915, pp. 411–13.

[35] Bancroft to E. B. Greene, Sept. 21, 1915; to Bowen, Sept. 28, 1915, AHA Papers, box 370. The vote in the council was fifteen to nine, with four abstaining, but most of the nine who voted against the appropriation were following the treasurer's advice to delay until the meeting in November, when the $400 would appear on the budget for 1916. Bowen to Greene, Sept. 21, 1915, *ibid.*, box 249, folder "Committee of Nine."

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by Dunbar Rowland when he signed a letter sent to prospective insurgents with his title as president of the Mississippi Valley Historical Association. His advice—to vote against Burr by writing "No Ring Rule" and the name of a rival candidate on the ballot—was predictable, and so was the storm of criticism. Moderates, offended by such lack of taste, either voiced their discontent to the *Nation,* as did Frederic L. Paxson in a spirited letter, or resigned from the Mississippi Valley Historical Association, as did Claude H. Van Tyne with the announcement that he preferred "ring rule" to "mob rule with demagogue leadership."[36] The Bancroft-Rowland clique seemed to be on the point of dissolution.

Bancroft, perhaps sensing this, stepped up his campaign to convince the doubting Thomases by gathering more evidence against the "ring." Once more statistics on expense accounts were demanded from annoyed officers, and once more each was checked against information solicited from railroads. Again a barrage of questions made life miserable for Clarence W. Bowen: what was the deficit for each of the last five years? could the council legally appropriate funds or only recommend appropriations to the annual business meeting? why were unexpended funds allowed to accumulate in some accounts when money was so desperately needed? why did Bowen continue to allot funds for the council's use when he was going to be defeated by the insurgents' candidate for treasurer in December? These were impertinent inquiries, but they unearthed not one whit of evidence of wrongdoing. The association's inner circle might indulge in $3.25 luncheons, but they were absolutely honest. Yet so pernicious was Bancroft's campaign that Henry Morse Stephens traveled from California to Washington at his own expense to deliver his presidential address that December, rather than embarrass the association with a request for the reimbursement to which he was entitled.[37]

Bancroft unearthed no new evidence of perfidy that fall of 1915, but he was still hopeful that the rank-and-file association members were behind him and would oust the scoundrels at the December business meeting, two especially. One was Burr, whose defeat for the presidency was a principal objective; Bancroft failed to find a rival candidate when his own choice, William Roscoe Thayer, refused to run, and so was forced to recommend Edward Channing, who had earned that dubious endorsement only by remaining neutral. The other was J. Franklin Jameson, editor of the *American Historical Review.* But how? Jameson was a superb editor who had made of the *Review* a model scholarly journal. Bancroft's answer was to attack the office rather than the man. If he could convince the business meeting that the association, not the board of editors, owned the magazine, he could manipu-

[36] Rowland, "To All Reformers in the American Historical Association," undated. A copy is in the Rowland Papers. Paxson's letter is in the *Nation,* Oct. 21, 1915, p. 495; Claude H. Van Tyne's letter, addressed to Leland, Nov. 1, 1915, is in the AHA Papers, box 31, folder "U–V."

[37] Bancroft to Bowen, Oct. 9, 14, Nov. 16, 1915, AHA Papers, box 370; H. Morse Stephens to Greene, Nov. 18, 1915, *ibid.,* box 249, folder "Stephens."

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late the executive council (by that time packed with his followers) into naming an editor more to his taste.

But how did one find proof that the *Review* was legally the property of the association? To Bancroft's suspicion-haunted mind the truth must be buried in documents the editorial board was deliberately hiding from the profession. These he must see. He made his demands in two peremptory letters to Turner, secretary of the board, that October, one asking for access to all minutes of all meetings, the other for transcripts of all contracts between the editors and publishers touching on the financial obligations of the board. Turner was in an awkward position. There was no question that Bancroft should not see the minutes. To surrender them to an officer of the American Historical Association on demand would be tacit admission that the association owned the *Review,* and this was still a mute point legally. Too, the minutes were largely discussions of articles being considered for publication and to make them public would embarrass contributors. "I would," Turner wrote, "as soon open the doors of my house to a fire bug without first calling in neighbors as witnesses and summoning the police and fire departments."[38] The minutes would be shown no one, Bancroft was told, unless the full board at its November meeting ruled otherwise.

Bancroft's second demand posed a more touchy problem. The records of financial transactions between the editorial board and publishers were private property, but they contained nothing confidential and could do no harm if released. Turner was inclined to let them go, but not until after Burr and Cheyney were consulted—by registered letter, for such was the apprehension of the officers that all communication now was by registered, special-delivery mail. Burr and Cheyney agreed, but Jameson had a better plan. To truckle to Bancroft's demand would create the impression that he had browbeaten the board of editors into complying. Why not have Evarts B. Greene, who had just been elevated to the secretaryship of the board, send all pertinent contracts and information to all members of the executive council and the committee of nine? Turner, Burr, and Cheyney accepted this proposal gratefully, but they foresaw one difficulty. Readying and duplicating the documents would take a fair amount of time. Given Bancroft's nervous state of mind, any such delay might spark another explosion.[39] Trouble was to be expected.

It came in a telegram to Turner: "Unless before 6 P.M. Monday next I am given permission to inspect and copy contracts and names of founders and guarantors I shall publicly expose you and board's attitude." These records, Turner patiently explained, were being prepared for

[38] Turner to McLaughlin, Oct. 6, 1915, Turner Papers, TU box 25. Bancroft's letters to Turner, dated Oct. 5, 1915, are in *ibid.* Jameson's advice was sought by Turner in two letters written on the same day, Oct. 9, 1915, AHA Papers, box 283, folder "T."

[39] Bancroft to Turner, Oct. 14, 1915; Jameson to Turner, Oct. 15, 16, 18, 20, 23, 24, 1915, Turner Papers, TU box 25; Turner to Jameson, Oct. 15, 1915, AHA Papers, box 283, folder "T."

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distribution by Evarts B. Greene at the University of Illinois and would presumably be in the mail soon. This kept the peace for only a few more days, then: "Where are the copies of contracts supposedly in mail Twenty-fourth inst?" More telegrams; there had been a delay in Greene's office. More demands from Bancroft, more placating replies, before the packet reached Washington on November 1. The documents, Bancroft exulted, were just what he wanted. They showed that the editors had no financial liability for the *Review*. Therefore they did not own the *Review!* Victory was his![40]

Bancroft had tasted blood (or so he thought) and wanted more—the board's minutes and lists of founders and guarantors—at once. More telegrams. More peremptory letters. Members of the board were in a quandary. They had agreed to surrender the *Review* to the association if the committee of nine so recommended, and to defy Bancroft now would indicate that they were defending their ownership. Too, the minutes filled 141 single-spaced pages and would cost a great deal of time and money to copy. Supplying the lists of founders and guarantors would also be expensive and time consuming, for each must be consulted before his name could be released. Turner and some of his colleagues were willing to comply, if only to keep Bancroft occupied, but Jameson would have none of that. To bow before pressure would only generate more pressure. Better to stop all this nonsense now. That sensible view carried the day. "I will," Turner wrote Bancroft, "refer your letter of November 3rd to the Board of Editors for consideration at their next regular meeting in Thanksgiving week."[41]

SPARRING BEGAN AGAIN even before the executive council and board of editors assembled in New York for their late November meeting. The presidential dinner would be held as usual (over Bancroft's objections), but the luncheon would be at the Woodstock Hotel rather than the Metropolitan Club, with each member paying the $1.50 charged. Members were also reminded that they were entitled only to railroad and Pullman fare and that all other expenses must be met from their own pockets. Jameson grumbled his dissatisfaction at abandoning the pleasant hour at the Metropolitan Club—"To make such a change in the face of the enemy will be attributed to fear—which is precisely what it is attributable to"—but he was voted down.[42] So Bancroft's charges were much on their

[40] Bancroft to Turner, Oct. 23, 28, Nov. 3, 1915; Turner to Bancroft, Oct. 24, 28, 1915, Turner Papers, TU box 25; Jameson to Turner, Oct. 23, 1915; Turner to Jameson, Oct. 23, 1915, AHA Papers, box 283, folder "T."

[41] Turner to Bancroft, Nov. 5, 1915; Turner to Greene, Nov. 5, 1915, Turner Papers, TU box 25; Burr to Turner, Oct. 25, 27, 1915; Turner to Burr, Oct. 26, 1915, Burr Papers; Turner to Jameson, Oct. 24, 1915, AHA Papers, box 283, folder "T."

[42] Greene to Jameson, Oct. 18, Nov. 2, 1915; Jameson to Greene, Oct. 21, Nov. 27, 1915, AHA Papers, box 249, folder "Bancroft Correspondence: Jameson."

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minds as they gathered on the night of November 26, 1915, to feast well at the expense of the retiring president (without Bancroft, who was sulking in his room) and to prepare for the next day's fray. They knew that Bancroft had arrived loaded with information and invective, fresh from a council of war with his principal conspirators held in Washington two days before.[43] Both sides were ready for a showdown, with preliminary skirmishing scheduled for the meeting of the board of editors and the final battlefield the executive council.

The editors met first, listened to Turner describe his recent correspondence with Bancroft, and with blood pressure properly elevated adopted a series of resolutions: they would *not* comply with his demand for a report on relations between the *Review* and the Carnegie Institution; they would *not* furnish him copies of their minutes unless ordered to do so by the council; they would *not* prepare a list of founders and guarantors unless asked by the council and committee of nine. Then, their stand made clear, the editors listened approvingly to a report on the whole controversy prepared by Cheyney. This masterful document told the story of Bancroft's attack from its beginning, commenting on "the rapid progress of this succession of letters from mere incivility to gross discourtesy," refuting his accusations of extravagance, and denying that members had ever dabbled in association politics save as individuals. "It is no light matter," the report concluded, "to have created scandal where no scandal was, and to have put before the community a pamphlet so expressed that while but few sentences can be described as actual falsehoods, there are very many that are not straightforward statements of the truth."[44] Cheyney came as close as a gentleman could to calling Frederic Bancroft a liar, and his fellow editors agreed. They unanimously voted that the report be submitted to the executive council and printed for general circulation.

This report was the first order of business when the council assembled that afternoon of November 27. Cheyney read it rapidly, for he was embarrassed by its length, with Bancroft trying to interrupt and Henry Morse Stephens, who presided, reminding him that he did not have the floor. The debate that followed was bitter; for three weary hours Bancroft asked questions, levied accusations, and demanded concessions. In the end he secured only one: he would be supplied a copy of the revised report

[43] Bancroft to H. Parker Willis, Nov. 15, 1915, Bancroft Papers, box 4; Bancroft to Bowen, Nov. 22, 1915, AHA Papers, box 370. Latané was unable to attend the council of war due to illness; only John M. Vincent of Johns Hopkins, H. Parker Willis, and Eugene C. Barker of Texas, a newly elected member of the executive council, were present. Willis was secretary of the Federal Reserve Board and was used by Bancroft as a financial expert.

[44] American Historical Association, *Report of the Board of Editors of the "American Historical Review," to the Executive Council, November 27, 1915* ([Washington, 1915]), 11–12. Several drafts of this statement are in the AHA Papers, box 370. Rough drafts of the minutes of this board of editors' meeting, kept by Turner, are in the Turner Papers, TU box 56. Turner's letter to Bancroft, summarizing the discussion and announcing that he would not have access to the minutes, is Turner to Bancroft, Nov. 29, 1915, AHA Papers, box 283, folder "T."

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before it was mailed to members of the association so that he could prepare a rebuttal. After that faint victory he sat silent as the council listened to a resolution expressing "its full confidence in the efficiency and unselfish manner in which the board of editors have conducted the affairs of the *Review* since its foundation." With Bancroft and the members of the editorial board refraining from voting, the resolution was unanimously adopted.[45]

Bancroft had suffered a thumping defeat, but he was as full of bluster and bravado as ever. "I held my ground," he reported to his brother, "and with ridicule and defiance I was slowly able to break down their majority." Indeed the "ring" had played into his hands; his answer to the editorial board's report would be so devastating that it would carry the day. "The rule of Jameson, Turner, McLaughlin and Burr should end this year," he predicted gleefully.[46]

Bancroft's lurid apprehensions governed his actions during the next few days. Cheyney and Greene, he believed, planned to change the board's report before giving him a copy, thus robbing him of ammunition needed for his reply. Bancroft knew that Cheyney carried the document to Philadelphia when the meeting adjourned on Saturday night, with instructions to send a legible copy to Greene in Urbana for duplication and distribution. Cheyney would finish his copying by Sunday night; the corrected manuscript should be mailed Monday morning and reach Greene on Tuesday. Any change in this schedule would be a sure sign that the plotters were carrying out their evil designs. They must be stopped from making those changes! Bancroft could count on his brother to guard the ramparts in the West; that sensible man must have doubted Frederic's sanity when he received a telegram on Saturday asking him to have a trusty agent ready to speed to Urbana to capture a valuable document at a moment's notice. By Sunday Bancroft was so apprehensive that another wire to Greene demanded news of progress, even though he must have known that Cheyney had had no time to mail the report from Philadelphia. Greene's patient rejoinder—that he did not yet have the material but hoped to be able to send a copy by Wednesday—did no good; another telegram was fired back: "That seems slow. Please employ extra typist post tonight." Again Greene explained that he had nothing to send and would not until Wednesday at least. Unfortunately the report did not reach Urbana from Philadelphia until Thursday morning, where it was reproduced so efficiently that a copy was on its way to Washington by 2:30 that afternoon. When it had not arrived by Saturday morning, Bancroft was again burning up the wires. Letting Cheyney have the report

[45] The meeting of the executive council is described in Jameson to Stephens, Dec. 1, 1915, AHA Papers, box 31, folder "S." The printed minutes are in the *Annual Report of the American Historical Association for 1915* (Washington, 1917), 75–79.

[46] Bancroft to Edgar A. Bancroft, Nov. 29, 1915, Bancroft Papers, box 4; Bancroft to Boyd, Dec. 8, 1915, *ibid.*

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was "a grave and inexcusable breach of trust." The report finally arrived that afternoon, and without a single alteration.[47]

Now for the reply. Bancroft already had started on a letter for the *Nation* that would "clean up all this rubbish"; now he would add his latest evidence of the editorial board's crimes and release the lot in time to sway the annual meeting. Such was his emotional state by this time that even his fellow conspirators feared his immoderate language would do more harm than good; Dunbar Rowland urged him to wait until he was "in a less disturbed state of mind."[48] That sound advice was brushed aside, but the editors of the *Nation* showed more sense. Once more they were fearful that Bancroft's snarling language and personal invective would offend their readers; once more they told him to look elsewhere for a publisher; once more he wailed that the editors were spineless lackeys who had succumbed to pressure from Harvard.[49]

And once more Bancroft turned to a privately printed pamphlet to unseat the "ring." Not one, but two appeared in late November and early December, *The Attempt to Seize the American Historical Review* and *Misrepresentations and Concealments in Opposition to Reform in the American Historical Association*—both typical Bancroftiana that rambled through page after page of unsubstantiated charges and misleading quarter-truths. A few new distortions added spice for the readers. Frederick Jackson Turner had been elected president of the association only after the inner powers learned that he was to be invited to Harvard (actually his nomination to the second vice-presidency in 1907 assured his ascent to the presidency). The editors had prepared a "diatribe" against Bancroft, which was to be kept secret so they could spread word-of-mouth calumnies, but his heroic battle at the November council meeting forced them to print copies for all the membership. They tried to make changes that would discredit him, but after a week-long struggle he secured a copy and frustrated that plot. These fantasies were bolstered with page after page of figures showing the "exorbitant" expenditures of the "ring" for travel and meetings; even larger sums, he hinted, had doubtless been used "for purposes both unauthorized and unknown either to the Council or the Association."[50] As J. Franklin Jameson finished the last page he wrote: "Isn't this great business for an

[47] Bancroft to Greene, Nov. 29, 30, Dec. 4, 1915; Greene to Bancroft, Nov. 29, 30, Dec. 2, 1915; Edward P. Cheyney to Greene, Nov. 30, Dec. 1, 2, 1915; Greene to Cheyney, Dec. 1, 2, 1915; Greene to Bancroft, Dec. 2, 1915; Bancroft to Greene, Dec. 4, 1915, AHA Papers, box 370.

[48] Rowland to Bancroft, undated, Rowland Papers.

[49] Bancroft to Fuller, Oct. 3, Nov. 2, 5, 1915; Bancroft to Villard, Nov. 5, 1915; Bancroft to Antoinette B. Pierce, Nov. 19, 1915; Bancroft to Edgar A. Bancroft, Nov. 23, 1915, Bancroft Papers, box 4.

[50] Frederic Bancroft, *The Attempt to Seize the American Historical Review: Rejoinder to Professor Albert Bushnell Hart* (Washington, 1915), 19–36, and *Misrepresentations and Concealments in Opposition to Reform in the American Historical Association* (Washington, 1915), 39–61. Turner bound these pamphlets, together with other printed material arising from the controversy, into a small book, which is in Turner Papers, TU box 56. This book is heavily underlined, with occasional marginal notations.

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immortal soul in A.D. 1915, when the whole world is on fire and civilization is going to pieces?"[51]

THE MEMBERS OF THE American Historical Association who gathered at Washington's New Willard Hotel on December 28, 1915, for their annual meeting agreed with Jameson. Both sides came well prepared for the final act in their melodrama. Bancroft housed his cohorts in the nearby Albany Hotel, where they could be handy for consultation, then summoned them to a final strategy dinner at the Metropolitan Club on the night of December 27. The list was pitifully small—Professors Latané and Ralph V. D. Magoffin from Johns Hopkins, Matthew Page Andrews of Baltimore, W. P. Trent of Columbia, Charles H. Ambler of West Virginia—that was all. Dunbar Rowland's name was conspicuously absent; he had prepared a "model constitution" for the association based on the insurgents' demands, and Bancroft was so annoyed by this threat to his authority that no invitation was issued.[52] These were the few Davids who would challenge the Goliath that had enslaved the historical profession—or so they pictured themselves. The Establishment made fewer preparations. Some thought was given to replacements for Turner and Burr on the board of editors—Haskins, they agreed, would fit well into Burr's shoes and Paxson, Van Tyne, or Carl Russell Fish into Turner's—and some discussion took place on a candidate to challenge Bancroft's re-election to the executive council, but Turner put an end to such manipulations by pointing out that this was a decision for the membership, not the governing clique.[53]

The two groups clashed first when the executive council met on December 27, a day before the full meetings began. Routine business first: the report of the treasurer (a $250 surplus for the year), the naming of new committees, the question of raising the registration fee from fifty cents to one dollar (referred to committee). Six o'clock was approaching before the council could turn to the first issue that would show the way the wind was blowing: a motion presented by the insurgents limiting travel expenses. Someone had the good sense to propose adjournment for dinner before debate began, but this only postponed the inevitable. It began when the council reassembled the next morning and dragged on for hour after hour of acrimony and semantic exhibitionism until three motions were agreed upon. One, instructing the treasurer to pay only railroad and Pullman fares for meetings of the council and committees, was passed unanimously. Another, outlawing travel expenses for com-

[51] Jameson to McLaughlin, Nov. 18, 1915, in Donnan and Stock, *An Historian's World,* 186.

[52] Bancroft to C. S. Neale, Dec. 9, 13, 1915; to William P. Trent, Dec. 22, 1915; to Villard, Dec. 22, 1915, Bancroft Papers, box 4. Rowland to Bancroft, Dec. 11, 1915; Bancroft to Rowland, Dec. 15, 1915, Rowland Papers.

[53] Greene to Turner, Dec. 15, 1915; Dana G. Munro to Turner, Dec. 18, 1915; Cheyney to Turner, Dec. 22, 1915; Turner to Burr, Dec. 20, 1915, Turner Papers, TU box 25.

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mittee or board meetings held in connection with the annual convention, was also adopted, even though it was known to discriminate against the *Review*'s editorial board, which alone met at that time. A third, requiring chairmen of committees to report in writing to the executive council rather than attending, was laid on the table.[54] The insurgents had won on two important points and faced the business meeting with confidence.

Their expectations were soon dashed, for the five hundred historians who crowded the ballroom of the New Willard Hotel were in no mood to bow to Bancroft and his followers. Instead they had embraced a more rational medium for reform. The report of the committee of nine, distributed the day before, recommended every major innovation needed to democratize the association's government. Why waste time by listening to the Bancroft insurgents now? Support for the committee of nine's report was almost universal, and as it mounted sentiment favoring the insurgents dwindled to the vanishing point.

Members showed their mood when Cheyney reported for the board of editors. Speaking slowly, clearly, and in a dignified tone, and sparking his remarks with wit, he swept his audience before him. Every mention of Jameson or Turner or McLaughlin brought cheers; they cheered again as Cheyney ticked off his points: the editors had no desire to continue ownership of the *Review;* they had served as custodians of the journal only in the interests of the profession; even though legal title was theirs they were eager to surrender all claims whenever the association was willing to assume the financial risks involved. Jameson spoke next, underlining Cheyney's points. The tumultuous applause that followed him to his seat convinced Claude H. Van Tyne that the time was ripe for a decisive stroke. Asking for the floor, he moved that attacks made on the character and motives of certain honored members of the association during the past year "meet with our entire disapproval, and that we hereby express our full confidence in the men whose motives and conduct have been thus impugned." The shout of approval was almost unanimous. Latané was seen voting in the negative, but Bancroft had left the hall, and his followers did not dare buck the tide. Even "funny old Rowland," as Jameson called him, turned to face the audience, "bowing and smiling and dee-lighted at the harmonious progress of reform under his leadership," as he cast his vote for a resolution that everyone knew was a slap in his face.[55]

[54] A stenographic report of this executive council meeting, filling eighty-six pages, is in the AHA Papers, box 256, folder "Minutes of Executive Council 1915." A briefer version is printed in the *Annual Report, 1915,* 79–82.

[55] The printed minutes of this session of the business meeting are in *Annual Report, 1915,* 49–51. It was graphically described in an article by Sidney B. Fay in the *Nation,* Jan. 6, 1916, pp. 22–23, and in a letter from Jameson to McLaughlin, Jan. 4, 1916, in Donnan and Stock, *An Historian's World,* 189–90. A full account is also in Cheyney to Turner, Dec. 31, 1915, Turner Papers, TU box 26. Turner was unable to attend, having been called to the bedside of his critically ill brother, who later recovered.

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There was no question now of the fate of the committee of nine that Bancroft opposed so stoutly. Its chairman, Andrew C. McLaughlin, could not be there to receive the acclaim he deserved; he was so broken with overwork and so impoverished by paying for the stenographic help necessary to keep up with the committee's correspondence that he had neither the energy nor money needed to make the trip from Chicago. William A. Dunning, at one time ranked by Bancroft among the insurgents, proved an able substitute. One after another the recommendations were presented. First, a series of changes to democratize the business meetings: minutes of the executive council to be circulated in advance, written reports of committees to be available two weeks before the meetings, a full half-day alloted to discussion. Next, a number of amendments to the constitution: eight rather than six elected members of the council, former presidents to serve only three years, the functions of the council more rigidly defined, the base of power shifted from it to the business meeting. Finally a number of new by-laws: the nominating committee to be chosen by the business meeting, election only through a printed ballot, recommendations for officers to be solicited from the membership, additional nominees to be listed when supported by a petition signed by twenty members, travel expenses for the council restricted to one meeting a year. Here was a package that out-reformed the reformers. To the surprise of all Dunbar Rowland made the motion to approve and beamed his gratitude when the recommendations were adopted with scarcely a dissenting voice.

The last recommendation of the committee of nine was the most controversial of all. Ownership of the *American Historical Review,* the committee reported, posed problems of finance, but it still recommended that the association assume the burden. Debate was spirited but generally favorable, with such old enemies as Jameson and Rowland voicing strong support. In the end a series of delaying amendments were shouted down, and the resolution was approved almost unanimously.[56] The committee of nine had accomplished reforms wanted by most of the members and dealt a death blow to the insurgents.

The hour was late now, but one more nail must be driven into their coffin. The nominating committee reported that 250 of the 325 members who had replied to their questionnaire favored George Lincoln Burr for the presidency to only 12 for his nearest rival. Worthington C. Ford had also far outdistanced all others for the first vice-presidency, but votes for the second vice-presidency were too scattered to be meaningful. Hence the committee recommended William Roscoe Thayer for that post. Of five candidates for the executive council, four—Eugene C. Barker, Guy Stanton Ford, Charles H. Haskins, and Ulrich B. Phillips—had received

[56] The minutes of the second session of the business meeting are in the *Annual Report, 1915,* 51–54, and the report of the committee of nine in *ibid.,* 69–75. Many of the recommendations of the committee of nine required constitutional changes that could not be finally approved until the meeting in 1916. All were adopted at that time.

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more than one hundred votes each and one—Frederic Bancroft—78. These were nominated together with Lucy M. Salmon. In addition Samuel B. Harding had been placed in nomination by petition, and the business meeting must elect six from this slate of seven. As ballots were being distributed Latané rose to announce that he had been authorized to withdraw the name of Frederic Bancroft. The remainder were elected.[57] All was over now save for one farcical anticlimax. Dunbar Rowland, still seeking a place in the sun, asked the floor to launch a rambling speech in which he claimed victory for the reformers, expressed surprise at the ease of their victory, congratulated his old enemy Burr, and pictured a glorious future for the association. His melodramatic language, his grammatical lapses, his bumbling delivery no less than his insincerity would have rocked the meeting with laughter had he not been so pathetic. By 6:30 at night, after four hours in hard chairs, the members were in no mood for pathos.[58]

THUS ENDED WHAT JAMESON called the "amusing tale of the insurgents of 1915—so eager for the maintenance of the Eighth Commandment that they forgot all about the Ninth."[59] Frederic Bancroft, John Latané, and Dunbar Rowland—"the Three Misquoters," as Turner liked to call them—had stirred a teapot tempest that left them with few friends, robbed them of their prestige, and doomed them to the ridicule of their colleagues. Their sincerity cannot be doubted, but all were painted with the tar brush by Bancroft's unrestrained invective, his picayunish concern with expense accounts ("his war," as his nephew put it, "against some indigent history professor who had recklessly squandered a dollar and twenty-three cents on a Pullman car somewhere"),[60] and his inability to realize that self-seeking was not the only motive governing human actions. Bancroft and his followers had done themselves irreparable harm, but they had also stirred a lethargic institution into overdue reforms that brought it into step with modern times. In retrospect the historical profession owes them a vote of gratitude that would hardly have been favored by Jameson or McLaughlin or Burr or Turner at the time.

[57] A copy of the printed ballots for this meeting, and the report of the nominating committee, are in Turner Papers, TU box 59.

[58] Two of Turner's former students painted this picture of Rowland in letters describing the meeting: Kenneth W. Colgrove to Turner, Jan. 6, 1916, and Arthur H. Buffinton to Turner, Jan. 9, 1916, *ibid.*, TU box 26.

[59] J. Franklin Jameson, "Early Days of the American Historical Association, 1884–1895," *AHR*, 40 (1934–35): 7–8. Readers less well versed in the Scriptures than Jameson might appreciate knowing that the Eighth Commandment reads, "Thou shalt not steal," and the Ninth, "Thou shalt not bear false witness against thy neighbor."

[60] Elbridge Pierce to Edgar A. Bancroft [undated], quoted in Cooke, *Frederic Bancroft*, 102.

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# T. S. Kuhn's Theory of Science and Its Implications for History

DAVID A. HOLLINGER

NOT SINCE THE PUBLICATION of R. G. Collingwood's *Idea of History* has a work of "theory" won from historians the amount of interest recently accorded Thomas S. Kuhn's *The Structure of Scientific Revolutions*.[1] If historians are conventionally aloof from philosophy of history, they are even less attentive to philosophy of science—yet contemporary footnotes prove that Kuhn's theory of *science* speaks to, and for, historians as few works of philosophy of *history* ever have. Even the revered Collingwood, for all his influence upon intellectual historians during the 1940s and 1950s, served to stop discussions as often as to advance them; a citation to Collingwood's profound but forbidding "Epilegomena"[2] enabled historians to perform an act of calm defiance: "we historians are on to something basic and complicated about human experience, which you can read about in Collingwood, and if you can't understand what he says, well, that's your problem." This defiant use of Collingwood may have been appropriate in some cases, and Collingwood will presumably continue to serve historians in this way without coming down off the shelf he now shares with a more mobile junior partner. Kuhn, unlike Collingwood, is being read carefully by many practicing historians.

Not since the time of Charles Beard has any guild historian attracted an audience among the academic intelligentsia as extensive as Kuhn's. Collingwood, too, was a historian, but he always remained the peculiar possession of historians and of a handful of philosophers; *The Structure of Scientific Revolutions* has become a major text for interdisciplinary discourse and has been acclaimed by the *cognoscenti* that reads Lévi-

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[1] R. G. Collingwood, *The Idea of History* (New York, 1946); Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962; 2d ed. 1970), 1.

[2] Collingwood, *Idea of History*, 205–334.



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Strauss, Piaget, Erikson, Laing, Lucács, and Chomsky.[3] Kuhn's audience beyond history and philosophy comes to him for what he says, or implies, about the relation of permanence to change, knowledge to culture, and history to value. This interest in Kuhn constitutes, among other things, a quickening of interest in insights that have long been vouchsafed to historians, for Kuhn's vision of these relationships owes much to the professional subculture of history. Hence Kuhn's relation to historians is simultaneously that of outsider and insider; he is a philosopher of science from whom historians can learn and a historian who may help clarify the historian's outlook for the benefit of an era that has long since turned to sociologists, scientists, psychologists, and literary critics for definitions of whatever issues engage the intelligentsia as a whole.

The meaning Kuhn's work has for history can best be clarified by answering three questions. First, To what extent can Kuhn's description of the behavior of scientific communities function heuristically, as a methodological postulate, in the study of communities organized for purposes other than that of doing science? More specifically, How, if at all, can Kuhn's sense of historical development enrich political, cultural, and intellectual history and other fields outside the history of science? Second, What are the normative implications of Kuhn's philosophy of science—his sense of validity—for the attempt to improve the quality of the "knowledge" historians produce? Third, What stake, if any, do historians in general have in the controversy between Kuhn and many philosophers over the nature of science?

*The Structure of Scientific Revolutions* excites the imagination of working historians chiefly because much of what it says about scientific communities seems to apply so strikingly to other kinds of communities. David H. Fischer asserts that Kuhn is "relevant to all fields" of history, and any number of manifestoes have announced the applicability of Kuhn to general intellectual history and its subdisciplines.[4] In practice, Kuhn's terms have been employed explicitly by historians of art, religion, political organization, social thought, and American foreign policy, in addition to their more predictable use by historians of the social sciences and the

[3] See, as examples, the references to Kuhn in Arthur Koestler and J. R. Smythies, eds., *Beyond Reductionism* (Boston, 1970), especially 228, and in Jean Piaget, *Structuralism* (London, 1971), 132. Cf. Karl W. Deutsch *et al.*, "Conditions Favoring Major Advances in Social Science," *Science*, 171 (1971): 450–59.

[4] David H. Fischer, *Historian's Fallacies: Toward a Logic of Historical Thought* (New York, 1970), 162; J. G. A. Pocock, *Politics, Language and Time* (New York, 1971), 15; George W. Stocking, Jr., *Race, Culture, and Evolution* (New York, 1968), 302; Harry W. Paul, "In Quest of Kerygma: Catholic Intellectual Life in Nineteenth-Century France," *AHR*, 75 (1969–70): 423; Arthur M. Schlesinger, jr., essay review of Robert A. Skotheim, *American Intellectual History and Historians*, in *History and Theory*, 7 (1968): 219–21; Hayden V. White, "The Tasks of Intellectual History," *Monist*, 53 (1969): 619.

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natural sciences.[5] It is Kuhn's sense of historical development that attracts all this attention; specifically, historians are moved by Kuhn's sense of what a tradition is, what conditions sustain it, and what the relation is between tradition and innovation. Kuhn acknowledges that his view of historical development owes much to the conventional historiography of politics and the arts,[6] but *The Structure of Scientific Revolutions* has made such a distinctive contribution that two of our most methodologically sophisticated historians of political thought—J. G. A. Pocock and Sheldon Wolin—have gone all the way to the history of science to find, in Kuhn, the theory of change satisfactory to them.[7] Most uses of Kuhn by historians overlook or minimize the prior kinship with Kuhn that in fact makes rapport so possible; Kuhn's terms are often imported, in toto, from their context in the history of science, so that Kuhn enters the discourse of historians as entirely an outsider, as yet another emissary from sociology whose newfangled ideas the historian feels obliged to summarize before "applying them to history." The result is that many "applications" of Kuhn take the somewhat incongruous form of analogies between science and nonscience; a Kuhnian "scientific revolution" is explicitly compared to the American decision to withdraw from Vietnam under the pressure of antiwar demonstrations at the Pentagon and at Chicago.[8]

The problem is that Kuhn's striking achievement in the history of science has made what we might call the species-specific aspects of his sense of historical development so available and compelling that we have difficulty getting through the species back to the genus of which Kuhn's theory is a part. Yet it is the generic in Kuhn that historians have been trying to employ, and this accounts for the fumbling apologies that so often accompany attempts to show that the history of society is, for all practical purposes, "just like" the history of tightly organized, technically

[5] E.g., James S. Ackerman, "The Demise of the Avante Garde: Notes on the Sociology of Recent American Art," *Comparative Studies in Society and History*, 11 (1969): especially 372; Murray G. Murphy, "On the Relation Between Science and Religion," *American Quarterly*, 20 (1968): 275–95; Pocock, *Politics*, 13–41; Sheldon S. Wolin, "Paradigms and Political Theories," in Preston King and B. C. Parekh, eds., *Politics and Experience: Essays Presented to Professor Michael Oakeshott on the Occasion of His Retirement* (Cambridge, 1968), 125–52; Reba N. Soffer, "The Revolution in English Social Thought," *AHR*, 75 (1969–70): 1938–64; Bruce Kuklick, "History as a Way of Learning," *American Quarterly*, 22 (1970): 609–28. Two examples of Kuhn's use in the history of social science are of special interest to historiography generally: Stocking, *Race*, especially 7–8, 70, 111–12, 232, 237, 302–03; Nathan G. Hale, Jr., *Freud and the Americans: The Beginnings of Psychoanalysis in the United States, 1876–1917* (New York, 1971), especially 71–115. This article is not concerned with the applicability of Kuhn's work to the field for which it was designed, the historiography of the developed sciences, but historians outside that field should be aware of the skepticism expressed by some historians of science. See, for example, John C. Greene, "The Kuhnian Paradigm and the Darwinian Revolution," in Duane H. D. Roller, ed., *Perspectives in the History of Science and Technology* (Norman, 1971), 3–25, but compare the persuasive defense of Kuhn by Leonard G. Wilson, "Commentary on the Paper of John C. Greene," *ibid.*, 31–37.

[6] Kuhn, "Postscript" to 2d ed. of *Structure*, 208; Kuhn, "Comment" [on the relation between art and science], *Comparative Studies in Society and History*, 11 (1969): 409.

[7] Pocock, *Politics*, 13–41; Sheldon S. Wolin, "Paradigms and Political Theories," 125–52.

[8] Kuklick, "History," 621.

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equipped scientific communities. Insofar as *The Structure of Scientific Revolutions* can enrich historiography beyond the sciences, insofar as the book's sense of historical development can function heuristically as a methodological postulate, it will do so less awkwardly if historians achieve a more comfortable relation with Kuhn, if they themselves seize more effective control over what they have found in Kuhn.[9] This requires a clear, general understanding of the sense of historical development that is embedded in *The Structure of Scientific Revolutions.*

Kuhn's notion of the "paradigm," his most celebrated and maligned term,[10] embodies the sense that activities are defined and controlled by tradition, and that tradition consists of a set of devices, or principles, that have proven their ability to order the experience of a given social constituency. An operative tradition provides a community with criteria to distinguish one activity from another, sets priorities among those activities, and enables the community to perform whatever common activities make it a community at all. Insofar as the community's common experience is contingent, that experience presents itself as a series of "problems" to be solved by the tradition, which validates itself by transforming the contingency of experience into something comprehensible and subject to maximum control. Tradition, then, is socially grounded, and its function is that of organization. Organization may be achieved through a number of modes and devices, ranging from formal institutions to informal habits and from codes of abstract principles to concrete examples of how problems of a given class have been solved in the past. Whether it is conduct or perceptions that require organization, whether the task is prescriptive or cognitive, the organizing devices have enough flexibility to sustain them through successive, contingent experiences; to the extent that a tradition can expand and adapt, like the English common law, it is that much more likely to retain its constituency.

But traditions do lose their constituencies sometimes, and it was of course the transition from one research tradition to another in scientific communities that Kuhn was especially concerned to explain. Kuhn's notions of "normal science," "anomaly," "crisis," and "paradigm-shift" (or "revolution") manifest an integrated set of senses concerning the relation of tradition to change. Change is possible within the terms of an operative tradition, as we have seen, insofar as the elements of the tradition are,

[9] Pocock, *Politics,* 13–41, is without question the most sophisticated and successful contribution to this effort yet to appear. Cf. the interesting essay by Randolph Starn, "Historians and 'Crisis,'" *Past and Present,* no. 52 (Aug. 1971): 3–22, especially 17–18.

[10] One ostensibly friendly reader claimed to find twenty-one meanings for this word in *The Structure of Scientific Revolutions:* Margaret Masterman, "The Nature of a Paradigm," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, 1970), 59–89. Kuhn has subsequently distinguished between two senses of "paradigm": (1) the "disciplinary matrix" consists of "the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community," including (2) "exemplars," the specific, "concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science." See "Postscript," 175, 182, 187.

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like principles or precedents in the common law, able to expand their implications enough to deal with new experiences while not losing their identity. Such innovation within a tradition is energized by an essentially conservative instinct, to maintain the viability of tried-and-true ways of acting and thinking. However tenacious a tradition, its constituency may still find itself surrounded with problems that defy solution. Traditional organizing devices, even when stretched, may fail to control and comprehend the experiences that apparently fall within their province. This failure can occur in a variety of ways. The community may be suddenly subject to conditions radically different from those in effect when the reigning traditions were institutionalized; another culture may have set up housekeeping next door, thereby creating a constant source of novel stimuli too immediate and concrete to be ignored even by those who would prefer not to acknowledge the novelty's existence. Or the community's activities in one area may be affected by upheavals in another: perhaps a political upheaval, replacing one governing elite with another, or replacing oligarchy with democracy, will produce new criteria for what counts as the satisfactory understanding and control of otherwise unchanged religious or economic life. The discovery of problems unsolved by the tradition may even result from a dynamism within the tradition itself: endemic tenacity may extend and refine organizing devices to such a degree of precision that they can recognize as "unorganizable" something that cruder, less demanding devices might treat as routine.[11] Whatever the path to crisis, a community thus disorganized must come up with a way to put things in order, and here the pioneering instinct comes into play. Attempts to refurbish the old tradition are replaced by the conscious search for new and more functional devices of organization; tenacity and singleness of purpose are replaced by the intentional proliferation of alternatives. A community's entire store of cultural resources may be ransacked before a consensus begins to emerge that certain proposed devices are superior to others. The more complete the consensus, the greater the stability the community enjoys, and the more likely it is that the new organizing devices will become traditional. Certain communities may go through a full cycle of (1) secure tradition, (2) novelty and confusion, (3) disagreement over whether to resist innovation or encourage it, and if the latter, in what direction, (4) coalescence around a candidate that might become (5) another secure tradition. Other communities may never achieve the unanimity necessary for this "cycle" to come about; rather their traditions may be less secure, their confusion and conflict more permanent, and their "revolutions" less pronounced, if ever worthy of the name.

Once this sense of historical development is abstracted from *The Struc-*

[11] Kuhn's concentration on this one factor is species-specific to *The Structure of Scientific Revolutions* and follows from the relative insulation of scientific communities from contingent social conditions external to the traditions of the community.

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*ture of Scientific Revolutions* it sounds like a set of truisms. This fact only serves to illustrate a point Kuhn has insisted upon: concrete examples, like Kuhn's achievement in the history of science, have a staying power distinct from that of the general principles they embody. Had Kuhn written the foregoing two paragraphs instead of having written *The Structure of Scientific Revolutions,* it is doubtful that he would have inspired so many attempts to "apply Kuhn's ideas to history." Yet the foregoing may enable historians to recognize and control just what it is that Kuhn seems to say for them. Once defined generically, the sense of development found in Kuhn can be more easily distinguished from other ideas about historical development and evaluated comparatively. And one man's "truism" can be another's enlightenment: theorists of social change, for example, now treat Kuhn as a significant and original contributor to that field.[12]

Among genera of theories of historical development, the one to which *The Structure of Scientific Revolutions* belongs acquires some of its distinctness from its indifference to the overall pattern and ultimate character of history. Connected to no total cosmic scheme, the theory stands somewhat apart from "speculative philosophy of history." There is no determinate life cycle, no implication that certain changes are "natural" for a cultural unit at certain times, no insistence that traditions will or will not attain a certain degree of stability, no reason, in principle, why a given tradition might not live forever. The theory neither holds that change is always gradual, cataclysmic, or dialectical nor insists that change is generated by elements exclusively within or outside of "social systems." The theory offers a thoroughly nonteleological view of change: no idea of progress is implied, nor one of decline; there is no sense that Athenian political organization was superior in the second century B.C. to what it had been in the fifth, no sense that New England's religious ideas were better, or worse, in 1730 than in 1630. No golden age stands behind or beyond, no *élan vital* energizes history, and no deities preside over it. Hence the generic sense of development that informs Kuhn's work is aloof from many of the concerns that drive the classical theorists surveyed recently by Bruce Mazlish and Robert A. Nisbet.[13]

Kuhn's general vision of change carries with it a minimum of anthropological and social theory. It does not specify as "natural" any particular relationships among the various psychosocial and economic drives that are served by organizing devices; it does not connect art and religion with

[12] E.g., Guy E. Swanson, *Social Change* (Glenview, Ill., 1971), 119–22; Isaac Kramnick, "Reflections on Revolution: Definitions and Explanation in Recent Scholarship," *History and Theory,* 11 (1972): 26–63; Robert A. Nisbet, *Social Change and History: Aspects of the Western Theory of Development* (New York, 1969), especially 324; and the attempt by the editors of *Comparative Studies in Society and History* to make Kuhn central to a discussion of "A General Theory of Innovation," 11 (1969): 369–432, especially 369.

[13] Bruce Mazlish, *The Riddle of History: The Great Speculators From Vico to Freud* (New York, 1966); Nisbet, *Social Change and History,* especially 303–04.

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economic needs, for example, nor does it suggest that all human activities have their being within a grand struggle between Eros and Thanatos. It depicts stability, equilibrium, and integration as goals, but these echoes of functionalist sociology are muted somewhat by the absence of anything like that school's emphasis on stasis: "the purposive end" of social systems, for the functionalists, is "the mere persistence and perpetuation of the system itself."[14] The Kuhnian vision emphasizes instead the terms on which a tradition justifies itself or loses its binding power: according to its ability to organize the community's contingent, historical experience. What is left open in the Kuhnian theory, considered generically, is the fundamental constitution of what is being stabilized and integrated. The aims of natural science help fill this gap for Kuhn in *The Structure of Scientific Revolutions,* but these are clearly "species-specific."

While the limited scope of Kuhn's general sense of development is the basis for much of its distinctness and utility, this cuts both ways. A historian concerned to explain a given temporal episode might find that Kuhn's vision frees the historian from grand developmental formulas and enables him to focus more directly on that episode, in its particularity; yet this same historian must look beyond Kuhn for a body of theory that will help him formulate and answer the questions left unasked by the Kuhnian framework. The latter simply would not predict, for example, that internal conflicts over the adequacy of competing religious faiths will be found to be functions of economic conditions. But neither would the Kuhnian vision prevent the discovery, through auxiliary means, that this was the case in a particular community at a particular time. Historians must be prepared to see things Kuhn does not lead them to look for, but Kuhn's sense of historical development may be able to neutralize the biases of a number of social and anthropological theories without excluding them from the matrix of inquiry. Kuhn's sense of development is different in scope and emphasis from Marxist, Freudian, and even functionalist theories, but its relation to them is not necessarily that of a rival: it can serve as a control, as a qualifier, whereby the traditional and fruitful eclecticism of the historian can continue to draw upon diverse insights about the nature of man and society without being victimized by attendant prescriptions concerning the nature of change. To the extent that historians employ generalizations in order to understand the particulars of history,[15] historians are best served by a sense of development like Kuhn's, which excludes the fewest of possible relationships from vision and retains the most.

[14] Kramnick, "Reflections on Revolution," 48, whose discussion of functionalism I have found helpful.

[15] On the role of theoretical "generalizations" as opposed to "laws" in historical inquiry, see Carey B. Joynt and Nicholas Rescher, "The Problem of Uniqueness in History," *History and Theory,* 1 (1961): 150–62; Maurice Mandelbaum, "Historical Explanation: The Problem of 'Covering Laws,'" *ibid.,* 229–42; C. J. Arthur, "On the Historical Understanding," *ibid.,* 7 (1968): 203–16; and Rudolph Weingartner, essay review of Morton White, *Foundations of Historical Knowledge,* in *History and Theory,* 7 (1968): 240–56, especially 255.

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The distinctness and potential utility of Kuhn's sense of development are functions not only of its limited scope but of its positive attributes. In the dynamics of change, for example, Kuhn's emphasis is on the role of tradition in helping to define a given contingent experience and in responding to it; traditions are not passive entities, helplessly battered about by circumstance, capable only of adapting to a concrete, externally defined given. Yet the ordinance that cultural forms have over experience is not to be understood as the triumph of spirit over matter, nor even of "ideas" over "events": the Kuhnian vision replaces this source of materialist-idealist disputation with a dialogue between traditions and contingent experience, in order that the historian can more freely investigate the functions of cultural forms as organizing devices. One can say of Kuhn what Gordon Wood has said about Bernard Bailyn in another context: he brings together idealism and behaviorism by showing that men are as much "victims" of their ideas as beneficiaries of them; traditions prevent men from seeing in their experience phenomena that an alternative tradition might lead them to confront.[16]

Furthermore, Kuhn anchors traditions firmly in social subgroup constituencies, and thereby he distinguishes his work from history that inquires less rigorously into exactly who is served by given institutions and ideas. Constituencies can be identified with relative ease in the developed sciences, but this species-specific aspect of Kuhn's work serves to encourage historians in general to ask more insistently, "Whose experience in connection with what activity is being organized by what cultural forms?" Hence Kuhn both promotes and socializes the "radical contextualism" of much of contemporary intellectual history, in keeping with which great pains are being taken to see that the choices attributed to historical figures like Hobbes and Locke were authentically available to them, not imposed on their milieu by history's subsequent elaboration of alternatives to positions they took.[17] More sociological than metaphysical or psychological in orientation, Kuhn's vision of historical development depends absolutely on "communities," whose internal and external relations in any particular case require careful definitions.

Essential to Kuhn's sense of what communities do are his emphases on conflict and problem solving. Subgroups in a community may propose devices for organizing whatever activities they must perform in concert with the community as a whole; when this happens the relation between the subgroups is one of competition. The proponents of each device at-

[16] Gordon S. Wood, "Rhetoric and Reality in the American Revolution," *William and Mary Quarterly*, 3d ser., 26 (1966): 23.

[17] See especially Quentin Skinner, "Meaning and Understanding in the History of Ideas," *History and Theory*, 8 (1969): 3–53, which is offered as an attempt to apply to the history of ideas a "set of concepts" similar to that applied to the history of science by Kuhn and to the history of art by E. H. Gombrich, *Art and Illusion* (Princeton, 1960); Skinner, 7. Cf. John Dunn, *John Locke* (Cambridge, 1968); Stocking, *Race*, 1–12. Stocking discusses *The Structure of Scientific Revolutions* along with another contextualist classic, Joseph R. Levenson, *Confucian China and Its Modern Fate* (Berkeley, 1958–65).

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tempt to persuade or overpower opposing groups, and in the process they may refine their devices to the point that they become more obviously functional: in this sense, competition can be productive. The alternative devices are candidates for the job of "problem solving," a notion so dominant in Kuhn's sense of development that all other activities are, in effect, translated into its terms. The plausibility of Kuhn in the present context depends largely on whether one believes this translation can be effected comfortably, behind the scenes, without turning "problem solving" into another heavy, mechanical formula, and without ignoring aspects of a community's life that we regard as essential to its history.[18] Certainly the assertive effort of scientists to expand knowledge through solving the "puzzles" of normal science is "species-specific" to *The Structure of Scientific Revolutions,* but this manifests the more generic sense that contingent experience functions in relation to a tradition as a series of "problems" that must be solved in order that the activities of the community, whatever they are, may be carried out in an organized, satisfactory fashion. The notion that all thought and action participate in "problem solving" is of course a familiar one, at least since the time of John Dewey.

Finally, a positive aspect of Kuhn's sense of development is its emphasis on elements of tradition that are prior to, or even apart from, principles, laws, and other conventionally "rational" organizing devices. Certain specific, concrete achievements within the remembered history of a community may function as models for thinking and acting without first being transformed into abstract principles. Kuhn thus reinforces compellingly the historian's practice of looking for prototypes of this sort, especially when trying to explain the behavior of a community that seems not to be following any principles at all. For example, English colonists in America enslaved Africans long before the principle of lifetime, heritable, racial slavery was acknowledged by English Americans. The behavior of the colonists was influenced by precedents from social situations they regarded as analogous.[19]

If *The Structure of Scientific Revolutions* can inspire attempts to depict in its terms the history of nation-states, no wonder this work has led historians to compare their own guild to Kuhn's scientific communities. While not "a science," the discipline of history is at least an academically organized branch of inquiry; it resembles Kuhn's scientific communities more obviously than do many of the cultural units that are said to partake of the same pattern of historical development. Increasingly, historians offer new interpretations, or suggestions for new research, as "paradigm-

[18] But cf. Pocock, *Politics,* 14.

[19] Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, 1968), 3–98.

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proposals," and historians have begun to regard basic changes in common outlook as "paradigm-shifts."[20] The introduction of this vocabulary has been especially easy since histories of the profession have long been grounded, quite firmly, in some of the generic insights that inform *The Structure of Scientific Revolutions.* Conceivably historiography could profit by yet more attention to the role of certain concrete achievements in establishing professional traditions and to the role of these traditions in controlling the response of historians to changes in their own intellectual and social environment; but the impression persists that we already have an abundance of exactly this sort of information about ourselves. The best of our professional histories was written by John Higham, Leonard Krieger, and Felix Gilbert soon after Kuhn's book appeared, but the authors of *History* seem not to have required Kuhn's help to direct their attention to questions of this sort.[21] Hence, while one could no doubt deliberate at length over the extent to which Kuhn's analysis of the historical development of scientific communities can, or should, be taken to apply to the professional community of historians, this question is finally not very interesting. A much more important and difficult question presents itself as soon as we acknowledge one crucial distinction between our relation to our own profession and our relation to conventional objects of historical study.

What matters most about the discipline of history is its ability to distinguish good history from bad history, accuracy from confusion, truth from fraud. However diverse and complicated the aims of doing history, we are at least dependent upon this activity for knowledge about certain things. While we care about the ways in which history is relative, we care even more about the ways in which it is true. Suddenly the fact that Kuhn wrote about science becomes important, as it decidedly is not when his work is read for its general sense of historical development. To what extent does Kuhn's philosophy of science—his sense of validity—provide the discipline of history with prescriptions for the improvement of its services? This question is grounded, then, in our engagement with the community rather than in our detachment from it; and its pursuit requires that we cease to read Kuhn as we have until this point, as a describer of the behavior of groups and the individuals within them. We need now to look within and beyond Kuhn's sense of historical development to find his sense of validity, or objectivity.

[20] Representative examples include Gene Wise, "Implicit Irony in Perry Miller's *New England Mind*," *Journal of the History of Ideas,* 29 (1968): especially 579–81, and papers given on December 28, 1968, at the meeting of the American Historical Association in Boston: Gene Wise, "Paradigm Formulation in Recent American Studies," and J. Rogers Hollingsworth, "A Paradigm for the Study of Political History." C. Vann Woodward interpreted the paradigm concept more strictly in his presidential address in 1969, when he observed that history, unlike the sciences, had never been endowed with a ruling paradigm: "The Future of the Past," *AHR,* 75 (1969–70): 726. Cf. John Higham's careful use of the term in his *Writing American History* (Bloomington, 1970), 172.

[21] John Higham *et al., History* (Englewood Cliffs, N. J., 1965).

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The relevance to history of Kuhn's sense of how the sciences achieve validity is more complicated than the tired question, Is history a science? Kuhn's admirers among historians include those least inclined to discard history's traditional autonomy for the more scientific standing presumably afforded by methodological integration with the social sciences;[22] historians differ from the sociologists and political scientists who take up Kuhn as yet another model of science against which their own activities can be measured. The difference follows from history's conviction that it antedates and transcends social science, that history partakes of art as well as science, that history is too much a "craft" to be subsumed under science. Historians have been less eager than social scientists to attribute to themselves the practice of "normal science" under controlling "paradigms," the mark of a truly "developed" science.[23] Yet historians, whether we regard them as craftsmen, protoscientists, or whatever, do operate with a sense that their discipline can be practiced with varying degrees of success. Hence historians can look to *The Structure of Scientific Revolutions* not to learn about the aims and methods of the sciences, but to understand how those aims and methods are fulfilled. The question is not the extent to which history and science share abstractly defined aims and methods, but whether Kuhn's account of how validity is achieved in the sciences can clarify and/or improve the status of validity in history. Historians continue to term good scholarship "objective," long after pretensions to "scientific history" have been forsaken. Can Kuhn's sense of validity help us, as historians, to clarify what we mean by "objective," and does Kuhn prescribe any means of achieving greater objectivity?

Certainly, without Kuhn's help, we could say that in the idiom of historians an "objective" account of some experience or event is valid on an intersubjective basis. This is to say that the truth of the account will be recognized by virtually all reasonable people, if they understand what is supposed to be the purpose of the account, if they follow the steps of its argument, and if they follow the pattern of its evidence. The account is about the event or experience, not about something else, and it is true, not false. Obviously there are questions begged in what counts as a "reasonable person" and what counts as "an experience or event," and these are among the issues about which philosophers argue. Positions on these issues divide one sense of validity from another.

[22] E.g., J. H. Hexter, "The Rhetoric of History," *History and Theory*, 6 (1967): 12–13, and Schlesinger, review of Skotheim, 219–21.

[23] Not all the social scientists who use Kuhn can be accused of simply wrapping themselves in "true science." An interesting example of restraint is Robert I. Watson, "Psychology: A Prescriptive Science," *American Psychologist*, 22 (1967): 436–40. Cf. David Truman, "Disillusion and Regeneration: The Quest for a Discipline," *American Political Science Review*, 59 (1965): especially 865–66. Yet even Truman, in his final paragraph (p. 873), seems to lean in the direction of the caricature of Kuhnian social scientists offered by Paul Feyerabend, "Consolations for the Specialist," in Lakatos and Musgrave, *Growth*, 198. Cf., as examples without Truman's restraint, Gabriel Almond, "Political Theory and Political Science," *American Political Science Review*, 60 (1966): especially 869, 875, and Robert T. Holt, "Comparative Studies Look Out-

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One such division is between Kuhn and his critics among philosophers of science, some of whom insist that Kuhn has no sense of validity at all. Kuhn, it is charged, has so relativized even the developed sciences as to deny their claims to objectivity; Kuhn's philosophy of science allegedly turns the decisions of scientific communities into matters for "mob psychology."[24] This reaction is sparked by Kuhn's profoundly sociological orientation. His sense of validity is essentially the following: a truth-claim becomes valid when the most learned practitioners of a technically sophisticated field agree that the theory on which the claim is based explains the range of phenomena under common scrutiny more satisfactorily than does any other known theory. This view of what counts as true differs from any simple correspondence-theory, according to which truth is a function of correspondence with something "out there." It also differs from the "justificationist" philosophies of science, according to which science is distinguished by its use of certain standards of rationality that are not culture-bound. But for Kuhn the crucial questions are: Whose word do we take concerning what's "out there" (in other words, which theory explains the relevant phenomena the most satisfactorily?), and whose sense of "satisfactory" do we employ? In both cases Kuhn's answer is that we take the word of the professional community; but when he says this he is referring to tightly organized, self-contained groups of experts bound together by rigorously defined questions and highly technical methods. Only these professional communities in the "developed sciences" are given this extraordinary position "above the law," above all models of justification. Kuhn can ground objectivity itself in professional communities of this sort because they alone have produced an impressive supply of ideas about nature that work. Ideas "work" when they predict the behavior of natural phenomena so well that we are enabled to manipulate nature or stay out of its way. Within the store of ideas of this type, some "work" better than others, and the successive discovery of better and better ideas is the transition from one valid idea to another, not a transition from invalidity to validity. The fact that a truth-claim may be brushed aside by a later, more technically equipped generation does not detract from that original truth-claim's status as "valid science for its own time."[25]

What needs to be underlined here is that Kuhn's sense of objectivity,

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ward." in Fred W. Riggs, ed., *International Studies: Present Status and Future Prospects* (Philadelphia, 1971), especially 134–36.

[24] E.g., Dudley Shapere, "The Paradigm Concept," *Science,* 172 (1971): 706; Israel Sheffler, *Science and Subjectivity* (Indianapolis, 1967); Karl Popper, "Normal Science and Its Dangers," in Lakatos and Musgrave, *Growth,* 56–57; Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *ibid.,* especially 93, 115, 178–79. Cf. Alan Ryan, *The Philosophy of the Social Sciences* (London, 1970), especially 233–35, and Peter Munz, essay review of Robert W. Friedrichs, *A Sociology of Sociology,* in *History and Theory,* 10 (1971): especially 364.

[25] This paragraph is based primarily on Kuhn, "Reflections on My Critics," in Lakatos and Musgrave, *Growth,* especially 238, 247, 254, 261–64. Cf. Kuhn, *Structure,* especially 144–59, and Kuhn, "Postscript," 198–200, 209–10.

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while it depends on agreement, is not entirely circular, as would be the case if the community's decision were taken in a vacuum. Rather the community has to take some account of the particulars that are observed under specified conditions. By thus acknowledging that there is something "out there" we do not reactivate the crude realism of the correspondence-theory; rather we recognize the obvious fact that any organized enterprise, however elaborate its working apparatus, must come to grips with its material conditions: politicians in need of money can be expected to accumulate a certain amount of knowledge about the political values of the rich. Surely there are no insuperable philosophical difficulties in recognizing that physical phenomena have considerable influence over what physicists do, that millionaires affect much of the behavior of impecunious politicians, and that primary documents from the reign of Henry II exercise some control over what Angevin constitutional historians do. As for how we know that a given glob of reality is, in fact, "physical," "a millionaire," or "a document from the reign of Henry II," it ought to be sufficient to say that while these judgments are, technically speaking, interpretations, only the most hopeless solipsist would deny that some "globs of reality" admit more easily than others of the sets of definitions on which physics, capitalism, and constitutional history are founded.

Now, what about the relation of Kuhn's sense of validity to the discipline of history? Insofar as historians have produced a body of knowledge that "works" to the satisfaction of everyone who cares, it consists largely of the semiautonomous, name-and-date "facts" that take up the pages of standard biographies, and that are only incidental to the questions historians try the hardest to answer. The most strikingly successful "answers" to questions historians care about are proverbially and persistently unable to inspire the degree of support given to reigning doctrines in the natural sciences—even the unusually tight "research consensus" created by C. Vann Woodward's *Origins of the New South* cannot serve as evidence that history is one of those disciplines that are "beyond the law." Historians, including those within fairly narrow subfields, are continually involved in debate over exactly what questions should be asked—precisely the sort of discussion the developed sciences are freed from by their tightly constructed "paradigm." Historians, moreover, actively go about asking radically different kinds of questions, and they grant professional status to work controlled by a number of different ideologies and commitments. Grudging or not, this tolerance of diversity distinguishes history from Kuhn's developed sciences, which tolerate it only when they do not know what they are doing, when a "normal science" consensus breaks down. Furthermore, the very notion of a "professional" community applies only ambiguously to history and to some of the social sciences, for the constituency of these disciplines is hard to define. In history, especially, the

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evaluation of a scholarly work frequently involves the participation of readers who, while not "professional historians," are sufficiently cognizant of what historians do to make the latter seek the approval of this larger, more intellectually diverse constituency.

Fortunately Kuhn's sense of validity has a corollary for knowledge-producing communities that fall short of the tightly organized, clearly successful research consensus of the developed sciences. These "proto-sciences," as Kuhn calls disciplines like history and most of the social sciences, both generate and test—however imperfectly—"testable propositions."[26] The professional community of historians regularly applies inter-subjective standards to the scholarship of its members. The "profession," even if this term is stretched to cover the educated nonprofessionals whose approval is important to historians, does make judgments, does provide an atmosphere of organized criticism. This remains true despite the fact that the touchstone of "good history" is notoriously difficult for anyone to define: the entire notion of intersubjective validity in history must live alongside the suspicion, if not the conviction, that the knowledge-producing aim of history is secondary to, or in any case qualified by, the moral and esthetic aims that presumably distinguish it from all sciences, physical and social. Indeed some historians would deny that the term "knowledge" is useful for denoting the type of "meanings" that historians discover in (or assign to) things. It would be a mistake to look to Kuhn for the clarification of these persistent complexities in the aims of history, but his view of validity is potentially relevant to history so long as we stipulate that the "testability" of a proposition is not a function of the use of any single, specific model of justification. However we define the aims of history, it remains true that no work of scholarship in that field will be counted as "successful" unless it persuades its professional readers of the following: (1) that the questions it asks are comprehensible, and worth asking; (2) that the sources it has examined are indeed the ones most relevant to the inquiry; and (3) that its analysis of the sources has been rational. By "rational" we mean that the author's presuppositions about human nature, the behavior of groups, causation, etc. are either shared by his readers or are perceived by his peers as respectable competitors to the views of the readers. The "knowledge" produced by historians is clearly distinguished from statements about the past that fail to persuade professional readers in these ways.

Hence we end up saying in a slightly different way something that has often been said about history: it is "imperfect knowledge." But when philosophers refer to history as "imperfect knowledge," they generally have the covering-law model of explanation in mind. In that view the knowledge produced by historians is imperfect because historians have not been able to explain particular events through the use of highly confirmed

[26] Kuhn, "Reflections," 244–45.

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"laws," as the cracking of a radiator on a cold night can be explained by general laws concerning what happens to water at different temperatures and to metals under stress. On the Kuhnian analysis, however, the covering-law model of explanation is merely one of a number of possible values that may form part of a discipline's working assumptions, and the presence or absence of complete hypothetico-deductive explanation is not, in itself, enough to establish the validity or lack of validity of a truth-claim presented in its name. This is where we can see Kuhn's sociological orientation the most dramatically. Kuhn might be willing to grant that most of the developed sciences do, as a behavioral fact, strive for hypothetico-deductive explanations, but "imperfect knowledge" in the Kuhnian sense is imperfect on the grounds of the extent of agreement about its truth and among whom it is agreed. The "explanation" that a community, or part of a community, accepts may derive its coherence from properties that cannot be translated, without remainder, into "laws" of any kind: the cogency with which vast amounts of material are made to fit together by a particular, concrete achievement like Perry Miller's *The New England Mind,* or, again, Woodward's *New South,* may persuade professional readers of a work's rationality.

Whatever the structure of rationality in the developed sciences, or in the "imperfect knowledge" of disciplines like history, the structure's sociological base has aroused in several philosophers the fear that Kuhn's effect is to encourage a protoscience to impose, arbitrarily, a tight research consensus upon its practitioners in order that the discipline might more nearly approximate the mature sciences. Kuhn, it is said, encourages intellectual retrenchment, pedantic specialization, the avoidance of logical rigor, and, if not mob rule, at least the unjustified refusal to join issue with arguments presented by professional minorities.[27] On this view Kuhn's implications for history would be as follows. In order to make their work more completely objective, historians may determine what kinds of questions can be answered without controversy and then confine research to these questions. The sure-fire means of getting reliable knowledge is the elaborating of truisms and the collecting of information that, however dubious its significance, is at least true. Or, if more ambitious questions are to be allowed, objectivity can be won by expelling from the profession

[27] For an especially willful expression of these fears, see Feyerabend, "Consolations," 198–99. Feyerabend's essay, incidentally, is one of the most trenchant critiques of Kuhn yet written. It and Margaret Masterman's "The Nature of a Paradigm" (59–89) are the most readable and interesting contributions to the Lakatos and Musgrave volume, which is a symposium on Kuhn's work and its relation to Karl Popper's philosophy of science. For a lively analysis of this volume and of its place in contemporary philosophy of science, see Joseph Agassi, "Tristram Shandy, Pierre Menard, and All That," *Inquiry,* 14 (1971): 152–64. The most reliable guide to the differences between Kuhn and the Popperians, however, is David Bloor, "Two Paradigms for Scientific Knowledge?" *Science Studies,* 1 (1971): 101–15. Bloor's sensitive analysis of how the Popperians, especially Lakatos, have revised their position to meet Kuhn's challenge helps to correct the popular misconception that it is only Kuhn who has been led to reformulate some of his claims since 1962.

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anyone unwilling to accept a given ideology. This would assure that research done by the remaining members of the group would not be subject to debilitating methodological criticism.

This reading of Kuhn's implications for history, and for any knowledge-producing discipline, is mistaken, I believe, and on grounds that demand clarification. Kuhn in fact presupposes a balance within professional communities between the drive to "perfect" their knowledge—even through retrenchment and the imitation of the developed sciences—and the drive to answer the very questions whose difficulty has prevented the prior achievement of a tight research consensus. Had Kuhn acknowledged this presupposition more openly, some of the confusion concerning his normative implications might have been avoided. Even when challenged to explain why his views do not encourage the hasty and arbitrary legislation of fundamentals and the repression of minorities Kuhn's response has been cryptic. The protosciences should be patient, Kuhn says, for maturity comes only to those who "wait and struggle" for it. But waiting and struggling presumably proceed on certain terms. What are they? And how shall "waiting and struggling" apply to crafts like history, which share only ambiguously in social science's quest for the "maturity" of the developed sciences?[28] Had Kuhn chosen to address these questions more directly, and had he outlined the normative implications of his philosophy of science for history, I believe he would have been bound to claim something like the following.

The professional community of history, or of any knowledge-producing enterprise, has available to it at all times the value-systems of other disciplines, including the developed sciences. Insofar as there is a loose consensus in the learned world as a whole about what it is to be "rational," history and similar communities are answerable to that consensus. As long as history claims to be a participant in learned discourse, history must maintain a substantial measure of rapport with what counts as "good sense" in this larger constituency. Within the learned world's vast store of vaguely compatible value-systems, historians will find a number of specific value-systems—including, for example, that embodied in the hypothetico-deductive model for explanation. Historians may take up these specific value-systems and see if they function well in the discipline of history, given the common aims that make it a community at all. Yet the "common aims of the community" are to some extent at issue when history, or any "protoscience," is trying to resolve a basic internal conflict or to choose among the various, specific value-systems available to the community. And Kuhn does not prescribe any *specific* standard for the resolution of such problems. Professional communities in the protosciences

[28] Kuhn, "Reflections," especially 245, where attention is directed only at the development of disciplines that seek to predict the behavior of natural phenomena and thereby attain "maturity."

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are not a "law unto themselves," to be sure, but they would seem to be under no more specific ordinance than the obligation to be reasonable. Kuhn's refusal to go farther than this, his refusal to endorse specific value-prescriptions for the protosciences greatly alarms his critics, but it makes sense once we recognize its connection to the trust Kuhn has in the motives men bring to inquiry in general.

Kuhn's *Structure of Scientific Revolutions* as well as his recent efforts to clarify the argument of that book reveal a willingness to grant legitimacy to the basic questions around which inquiry does, as a behavioral fact, develop. Kuhn seems to assume that physical inquiry, historical inquiry, philosophical inquiry, zoological inquiry, political inquiry, or whatever, whether or not they have become developed sciences, or whether they ever will, do possess a kind of primal validity: the drives that bring these inquiries into being provide their practitioners with something to stand on in terms of the aims of the professional communities that may form around these inquiries. Kuhn, therefore, does not prescribe basic aims for the branches of inquiry; these aims are there and can be trusted. Indeed Kuhn so took for granted the legitimacy of the aims of the developed sciences that at least one critic was moved to complain that Kuhn did not discuss "the aim of science." Kuhn's response to this has been that the developed sciences seek "to explain in detail a range of natural phenomena,"[29] an answer too vague, I am sure, to satisfy his critics. Kuhn is simply more willing than are many philosophers of science to entrust responsibility to people who are not philosophers of science. If this makes him an "irrationalist," I do not see how the charge can be refuted. The persons drawn to a given inquiry and acculturated into the organized community that has taken form around that inquiry are, Kuhn seems to imply, in a position to take the lead in evaluating the various approaches to their field that the surrounding learned world presents to the community. Kuhn assumes that these practitioners themselves are sufficiently loyal to their callings to look for answers that will neither abandon their basic questions nor repudiate what counts as "rationality" in the larger culture for whose benefit the inquiry is being conducted. Obviously, disciplines without a "normal science" consensus must live at all times in a state that approximates "crisis" in a developed science, and Kuhn assumes that the basic aims of such disciplines are compelling enough to enable their practitioners to endure the uncertainty and conflict that attend upon theory proliferation and energetic methodological criticism.

The scope of Kuhn's normative implications for history and other protosciences is thus extremely limited, encompassing chiefly the questions of who should decide what, to the relative satisfaction of whom? Some of the confusion about Kuhn's normative implications might have been

[29] Feyerabend, "Consolations," 201; Kuhn, "Reflections," 245.

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eliminated, again, had Kuhn more boldly distinguished between prescriptions of this sort, which are intrinsic to his theory of science, and prescriptions that are in principle consistent with that theory although Kuhn himself does not make them. The latter could include prescriptions of almost any specific content, so long as they are generated, discussed, and adopted or rejected within the appropriate framework of relations among practitioners, their colleagues, and their larger constituencies in the learned world. The fact that Kuhn has chosen not to integrate specific value-prescriptions for sociology or theology into *The Structure of Scientific Revolutions* must not be taken to obscure his conditional approval of interdisciplinary dialogue concerning the ways in which the basic aims of various disciplines ought to be interpreted and pursued.[30] Kuhn does, of course, separate himself from the schoolmarmish practice of the positivists, according to which the branches of knowledge are treated as pupils, some to receive gold stars for the mastery of certain methods, some to wear the dunce cap for speaking nonsense.

Perhaps Kuhn's position can be brought out more distinctly if we compare it to each of the polar attitudes that have defined the analytical philosophy of history during the past twenty-five years. As Rudolph Weingartner has pointed out, philosophers have been divided between those who wish to provide an account of what historians ought to be doing and those who wish to provide an account of what they actually do.[31] The most conspicuous of the former are the followers of Carl Hempel, the covering-law theorists whose philosophy of history provides historians with a specific model for explanation that, if it could be brought into more complete operation in history, would perfect the knowledge historians produce. Opposed to the "Hempelians" are a number of philosophers who take the methods of historians as self-justifying and who seek to provide an adequate philosophical account of what historians do. Historians almost always prefer the latter style of philosophy, for it can be taken to legitimize aloofness on the part of historians from methodological discussions in the social sciences and humanities generally. Historians interested in justifying this aloofness will not get much help from Kuhn, however, for he implies that history, insofar as it expects to survive as a recognized branch of inquiry, cannot afford to ignore frequently made methodological complaints about its procedures. Yet Kuhn does imply that historians have, in their own sense of their common aims, a legitimate place to stand when entering theoretical discourse with nonhistorians. For example, Kuhn would surely defend the reluctance of historians to put aside narratives, even though this reluctance creates ambiguities in

[30] Kuhn does make these prescriptions, of course, for the disciplines he practices, or is close to; see not only *Structure, passim,* but also "The Relations between History and History of Science," *Daedalus,* 100 (1971): 271–304.

[31] Rudolph Weingartner, "The Quarrel about Historical Explanation," in Ronald H. Nash, ed., *Ideas of History* (New York, 1969), 2: 140–57.

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"historical explanation" that diminish the standing of history in the eyes of some philosophers and scientists, who would prefer that explanation by covering laws proceed in a more forthright fashion.

If Kuhn would not regard the discipline of history as free from the need to justify itself, neither would he free subgroups of historians with special "perspectives" from the need to justify their point of view to other historians. We often speak of a historian's perspective as much more than a limitation on his objectivity; we see it rather as a positive opportunity to observe things that are obscure from other perspectives.[32] *The Structure of Scientific Revolutions* certainly supports this understanding of the function of a "point of view," but Kuhn's sociological sense of what makes an idea true exercises an important control on "perspectivism," and prevents it from turning into the more complete relativism of "every man his own historian." To the extent that what historians do has a claim to knowledge, this claim is based on the existence of a community, however amorphous, that evaluates the various "perspectives" of its members, as well as the relation between a given perspective and what is allegedly "discovered" within it. The community distinguishes among points of view that are comprehensive, parochial, and incoherent.

Community sanction is thus essential to knowledge, even when "imperfect," but this does not necessarily imply the repression of points of view the profession as a whole regards as "parochial." On the contrary, these ostensibly "parochial" perspectives have the same relation to the professional community of historians that the latter has to the other branches of knowledge. Just as historians can refuse to do away with "narratives" and still be considered, by most of the learned world, as producers of knowledge, so can the advocates of a "parochial" vantage point like Marxism remain doggedly "parochial" and still be contributors to history. But the Marxists, to take them only as an example, remain second-class citizens in the community of history in the same sense that history is a second-class citizen in a learned world where the developed sciences set the standard for knowledge. If Marxists refuse to abandon their parochialism in order to become better acculturated as historians, their situation is again comparable to the historians, in their entirety, in relation to the larger learned community: the parochial Marxists and the redoubtable narrative historians each have a set of commitments too essential to their callings to be relinquished, and in each case, also, the recalcitrance of the minority is tolerated by the larger group. The learned world tolerates narratives because it suspects that what works in physical inquiry might not, after all, work for every kind of inquiry, and the historical profession tolerates the Marxists because it knows that its collective judgment is too

[32] An excellent summary of what has been conventional wisdom for historians during the past twenty years is found in Higham *et al.*, *History*, 135–44, especially 136 (since the 1950s the emphasis has been on "the positive opportunities of the historian's observational position").

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"imperfect" to justify the expulsion of a parochial minority with as much empirical and theoretical foundation as the Marxists. In both cases, then, the minority is taken seriously because it can make a case for itself in terms that can be at least understood by the majority.

Does this suggest that the appropriate balance between the demands of minorities and majorities is achieved automatically? Does it mean that Marxist interpretations of history have gotten no more and no less than their share of attention from the profession as a whole, decade by decade, during the twentieth century? Would Kuhn prevent us from saying that professional communities make mistakes? Since the meaning of these queries depends on whether they are taken to presuppose a transcendent standard for judgment, we are led directly into Kuhn's attempt to release philosophy of science from the need for such a standard.

THAT THE HISTORY of a thing can tell you something about its nature has always been a controversial assertion with reference to science, an activity that even the nineteenth century was unable to historicize completely. Historians of science have been expected to write the history of an activity whose nature was known, or was in any case the business of someone else to define; not until recently have they been called upon to clarify the nature of science. Kuhn has been the most insistent advocate of this new role for history since 1962, when he introduced *The Structure of Scientific Revolutions* with the claim that the historical study of science "could produce a decisive transformation" in views on the nature of science. These words gained some of their drama from their immediate context: Kuhn's volume was published as part of the *International Encyclopedia of Unified Science,* the *summa* of logical positivism, the movement that viewed the nature of science as the most strictly synonymous with its logic. Kuhn sought to historicize the most recalcitrant of subjects, science, and he threatened to drive the "Whig interpretation of history" out of its last well-defended enclave, the historiography of the sciences.[33] Kuhn would carry the insights of historiography into new territory.

Kuhn's assault on Whiggery in the history of science met with complaints very much like those raised by defenders of Whiggish history in other fields. We have often been told, for example, that the important thing about antebellum abolitionists is that they were right. Whatever the complex web of historical conditions that enabled the antislavery

[33] Kuhn has not been alone in these efforts, as he acknowledges in *Structure,* especially 2–3, and in "History of Science," *International Encyclopedia of the Social Sciences* (New York, 1968), 14: 74–83. Cf. Kuhn, "The Relations between History and History of Science," 288–91. Cf. also two very helpful accounts of recent developments in the history and philosophy of science: Arnold Thackray, "Science: Has Its Present Past a Future?" in Roger H. Steuwer, ed., *Historical and Philosophical Perspectives of Science* (Minneapolis, 1970), 112–27, and Stephen Toulmin, "Rediscovering History: New Directions in Philosophy of Science," *Encounter,* Jan. 1971, pp. 53–64.

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radicals to see the evil of slavery clearly and to act to try to end it, our rational and moral relation to these radicals can only be obscured by attempts to bring out those sustaining conditions in their full, historical, material complexity. Accounts of the psychosocial basis for the behavior of the abolitionists detract from its righteousness, the argument goes, just as a sociological orientation toward science obscures the two things about science that are truly important: its ideal logic of justification and its access to the objective natural order. The ideal structure of science and the morally right response of reformers are what we need to understand; we cannot be helped by studies that justify obfuscation and debunking in the name of "comprehensiveness and complexity." Certain ethical and logical ideals are so important to our survival, and so precariously held, that they need reinforcement, not the more complete "understanding" that risks the miring of these ideals in the swamps of human nature and history.

Oliver Wendell Holmes, Jr.'s attempt to historicize law produced a discussion similar to the one now surrounding Kuhn's work. Holmes's belief that the law was made by judges, that its life was "experience" instead of logic, that there was no "natural law" waiting to be discovered and declared, placed him in opposition to the brittle formalism of his generation's jurisprudence.[34] To be sure, the "legal realists" who claimed Holmes as their prophet went on to deny the influence of precedent and reason, of rules and logic, on the behavior of judges,[35] but the realists' relation to Holmes is analogous to the relation of Kuhn's followers in the "counterculture" to *The Structure of Scientific Revolutions*.[36] Far from "irrationalism," Kuhn's view of science is remarkably like the conventional view of law, now that the Holmesian insights have been detached from the excesses of the realists: law (science) is part of culture, but culture brings great rational and moral resources to the improvement of law (science), and the lack of a transcendent standard does not endanger society's loyalty to law (science).

The controversy over Kuhn's work is even more strikingly reminiscent of the nineteenth-century *Angst* over the fate of the doctrine of design. In *The Structure of Scientific Revolutions* Kuhn predicted that the "main obstacle" to the historicization of science would be the same abstract

[34] For Holmes and his intellectual environment, see Morton G. White, *Social Thought in America: The Revolt Against Formalism* (New York, 1949), 59–75. The comparison of Kuhn to Holmes is also made in an essay I read only after this paper was well advanced, M. D. King, "Reason, Tradition, and the Progressiveness of Science," *History and Theory*, 10 (1971): 24–25.

[35] Wilfrid E. Rumble, Jr., *American Legal Realism: Skepticism, Reform, and the Judicial Process* (Ithaca, 1968).

[36] E.g., W. I. Thompson, "Alternative Realities," New York *Times Book Review*, Feb. 13, 1972, where the view that scientific discoveries are made by those "working outside the containers [of knowledge] in the dark of the unknown . . ." is misleadingly attributed to Kuhn. Cf. the equally dubious attempt to link Kuhn with the socioeconomic determinism of Engels and J. D. Bernal: James E. Hansen, "An Historical Critique of Empiricism," in David H. DeGrood *et al., Radical Currents in Contemporary Philosophy* (St. Louis, 1971), 44, 48.

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convictions marshaled against Darwinian natural selection.[37] The subsequent decade has confirmed the prediction. Since certain organisms and their component parts were so supremely capable of doing their job, how could biologists explain their development without design? Since certain scientific theories work so well, how can any explanation of their development dispense with a theory-independent, objective, natural order? Today most of us believe that Darwin's opponents exaggerated evolution's threats to the integrity of civilization and to the identity of man; the defenders of design seem to have been motivated by a sense of permanence much too extreme and absolute. The present issue is whether Kuhn denies to science the stability of structure and environment required for its practice. Kuhn's critics charge, in effect, that Kuhn is naive about how great and abiding is our need for cosmic anchors, for ideals of perfection unsullied by social, psychological, and historical functionality.

Kuhn's theory of science dispenses with the idea of a fixed, permanent natural order that can function both as a standard for truth in the case of particular theories and as a goal for the progress of science. Kuhn also rejects the a priori methodological unity of science, according to which specific, formalized rules of verification are assumed to attend upon the basic aims of science. Scientific progress for Kuhn is not progress toward completeness via the accumulation of correct observations; it is, rather, "evolution *from* primitive beginnings," from what scientists agree upon to explanations that increase and refine their "understanding of nature."[38] This "progress" of science is made possible by a conjunction of (1) continuity in basic aims,[39] with (2) the mysterious fact that parts of the natural world turn out to be "knowable,"[40] which is to say that the object of knowledge presents problems that can be solved to the satisfaction of enough people to enable a tight research consensus to come into being. This consensus, in turn, promotes the microscopic specialization that allows scientists either to expand the range of phenomena explained by their theories or to discover anomalies. The eventual explanation of anomalies may require the community to choose among alternative revisions of theory, and choices made by individual members of the community are controlled by a complex of preferences enumerated in no existing "logic of explanation." What the community decides will in any case settle the matter, will determine which theory revision will count as

[37] Kuhn, *Structure,* 171–72.

[38] *Ibid.,* 170.

[39] This continuity of basic aims (see *ibid.,* 168) is what I take Kuhn to be clarifying by his recent references to the "paramount" values of scientific communities. Kuhn, "Logic of Discovery or Psychology of Research?" in Lakatos and Musgrave, *Growth,* 21; "Postscript," especially 184–85; "Reflections," 262. These clarifications have been interpreted by some as an important change in Kuhn's position. E.g., King, "Progressiveness of Science," 29. In any case, further research on what scientific communities value, tolerate, and disdain is Kuhn's own chief desideratum. See "Reflections," 238.

[40] Kuhn, *Structure,* 173.

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a progressive step in science. Such is the theory of science that Sir Karl Popper believes to be giving comfort to the enemies of "our civilization."[41]

We must aim at truth, said the late Joseph R. Levenson, "even if the truth cannot be known."[42] The fate of Kuhn's theory of science depends partially on the extent to which this tension is in fact bearable. In the context of contemporary thinking about the cultural relations of science, Kuhn's work raises the following question: Is it a necessary condition of the successful pursuit of science that scientists, and/or the societies to which scientists look for support, retain the conception of a "fixed permanent scientific truth"[43] as the goal of science? Kuhn himself has not confronted this question directly, but *The Structure of Scientific Revolutions* surely assumes that our ability to make judgments can survive the knowledge of how entangled those judgments are in our psychosocial matrix and that neither our reason nor our values are inappropriately threatened by a thoroughly historical perspective. To the extent that we see around us certain disciplines that abandon their callings for the academic equivalent of get-rich-quick schemes, Kuhn trusts that such hoaxes can be identified and criticized without a transcendent standard. In this view culture-bound standards are stable enough to define "mistakes," to sustain a critical attitude toward our intellectual environment, so long as we understand that the transition from transcendent objectivity to socially grounded objectivity need not be a substitution of terror and caprice for rationality. *The Structure of Scientific Revolutions* can be read as an invitation to forsake at last the fictional absolutes of natural theology.

Issues of such depth would not be raised by the historicization of science if this process were allowed to proceed within the terms of the now-classical distinction between the historical sociology of scientific knowledge and the philosophy of scientific justification.[44] In keeping with this distinction, conventional sociology of science does not attempt to integrate its sociological explanations of ideas about nature with an articulate interpretation of what makes such ideas valid or invalid.[45] Questions about the success with which science achieves its aims are supposedly beyond the scope of the discipline, as, indeed, they would be beyond the scope of any historical, psychological, or sociological study of science, if the above distinction were rigorously adhered to. Yet *The Structure of Scientific Revolutions* seeks explicitly to explain the success

[41] Karl Popper, "Normal Science and Its Dangers," in Lakatos and Musgrave, *Growth*, 53; cf. Herbert Feigl, "Beyond Peaceful Coexistence," in Stuewer, *Perspectives*, 7.

[42] Levenson, *Confucian China*, 3: 89.

[43] The phrase is Kuhn's. *Structure*, 172.

[44] See, as examples of the formulation and use of this distinction, Herbert Feigl, "Philosophy of Science," in Roderick M. Chisholm *et al.*, *Philosophy* (Englewood Cliffs, N. J., 1964), 472, and Popper, "Normal Science and Its Dangers," 56–58.

[45] E.g., Bernard Barber, "The Sociology of Science," *International Encyclopedia of the Social Sciences* (New York, 1968), 14: 92–100; cf. Stephen Cotgrove, "The Sociology of Science and Technology," *British Journal of Sociology*, 21 (1970): 1–15.

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of science;[46] this work is clearly distinguished from historical approaches to science that ignore validity, or translate it into entirely neutral terms. Kuhn's attempt to account for the validity as well as the relativity of science endows his work with a significance for historians that will be missed by those who look only at his sense of development, and then at his sense of validity, without seeing how the two are actually related in *The Structure of Scientific Revolutions* itself. For the "science" that Kuhn would historicize remains throughout his work the real thing, the explicitly successful explanation of natural phenomena. Kuhn's "science" is an activity that we value greatly, an enterprise upon which "our civilization," as Sir Karl would say, and properly, is crucially dependent. Its historicization therefore brings into bold relief as nothing else now can the question of history's relation to value. Does a historical understanding of the "problem-solutions" we rely upon inhibit significantly our ability to defend, criticize, alter, and defend again those very "problem-solutions," those answers to life's questions that we believe in? Responses to this query have always affected the extent to which the insights of historians are solicited, ignored, or resisted. The discipline of history has a stake of its own in the controversy over the full historicization of science.[47]

[46] Kuhn, *Structure,* v, 160–73; Kuhn, "Reflections," 236.

[47] Two very important contributions to the controversy appeared just when this article was being completed, Stephen Toulmin, *Human Understanding: The Collective Use and Evolution of Concepts* (Princeton, 1972), and Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford, 1972).

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# Hitler and Psychohistory

A Review Article by HANS W. GATZKE

WALTER C. LANGER. *The Mind of Adolf Hitler: The Secret Wartime Report.* Foreword by WILLIAM L. LANGER. Afterword by ROBERT G. L. WAITE. New York: Basic Books. 1972. Pp. ix, 269. $10.00.

ADOLF HITLER IS once again much in the news. The London *Daily Mail* calls this "one of the best years Hitler ever had," and according to Hamburg's *Die Zeit,* "Hitler is *in.*" In England there has been a veritable rash of movies, television series, and plays about the German dictator, and such "entertainment" will no doubt soon spread to this country.[1] At the moment at least five full-length studies of Hitler have been announced, including one entitled *The Psychopathic God.*[2] Why, one wonders, this sudden quickening of interest? Is it, as some observers feel, because our age of highjackings, kidnapings, and murders can be traced back to "Hitlerism" and its cult of naked force and ruthless aggression? Or is the Hitler boom merely due to the success of Albert Speer's memoirs[3] and the realization that anything involving Hitler is sure of a large audience? The interesting part is that, with few exceptions, the preoccupation with the dead Führer has thus far not produced anything that cannot already be found in the still two best books about him, one of them written thirty years ago.[4] Among the exceptions is the work of an American psychoanalyst, Walter C. Langer, entitled *The Mind of Adolf Hitler.* It, too, is not really new—it was first written in 1943 as a report for America's World War II intelligence agency, the OSS. But publication, though belated, is still valuable, if only for the debate it will cause.

Dr. Langer's book is not the only work dealing with Hitler's psyche; that subject has long fascinated historians and psychologists alike.[5] But

1 Karl-Heinz Wocker, "Oscar für Adolf. Hitler-Boom im englischen Film und Fernsehen," *Die Zeit* (American ed.), Sept. 19, 1972.

2 By Robert G. L. Waite. The others are by Horst von Maltitz, John Toland, Rudolph Binion, and Joachim Fest.

3 *Inside the Third Reich: Memoirs* (New York, 1970).

4 Konrad Heiden, *Der Fuehrer* (New York, 1944), which deals with Hitler's early years until 1934; and Alan Bullock, *Hitler: A Study in Tyranny* (rev. ed.; New York, 1964), which covers Hitler's whole career.

5 For a listing and discussion of some of the relevant literature, see Wilhelm Lange-Eichbaum and Wolfram Kurth, *Genie, Irrsinn und Ruhm* (6th ed.; Munich, 1967), 381–88, 653–54.



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the book is the most detailed and, despite its many flaws, most thorough study of its kind published to date. The original OSS report was not declassified until 1968, and its existence was little known.[6] Even William L. Langer, in a foreword to his brother's book, states that he only saw it some time after it was completed, a surprising fact considering the well-known historian's prominent role in the OSS and his interest in psychohistory.[7] Also surprising is the fact that the published volume differs in many respects from the original Langer manuscript.[8] In his introduction Walter Langer tells of the pressure under which he wrote his report in 1943, finishing just ahead of the deadline set by the OSS. "The first draft," he adds, "automatically became the one and only draft," and he regrets that a revision of that draft was impossible.[9] Recent correspondence with the publisher, however, has revealed that the original manuscript was changed and edited several times by Dr. Langer and others, both in 1943 and again before publication. The claims made on the dust jacket, therefore, that "here is the secret psychological report written in 1943," and the statements by Walter Langer in his introduction and by Robert G. L. Waite in his afterword that the book presents a "historical document," are hardly justified.[10] Serious students of Hitler will still have to consult the original OSS report in the National Archives, where they will also find the massive "Hitler Source-Book" containing the raw material from which the analysis of Hitler's mind was made.[11]

Dr. Langer's introduction also tells how the original report came to be written. It seems that the book is actually the result of a collaborative effort by Langer and "three experienced psychoanalysts."[12] The contributions of Drs. Henry A. Murray, Ernst Kris, and Bertram D. Lewin seem to have been substantial. Dr. Murray of the Harvard Psychological Clinic in fact wrote his "Analysis of the Personality of Adolph [*sic*] Hitler" at about the time when Dr. Langer was writing his own report.[13] Yet neither Dr. Murray's name nor those of his two colleagues appear in Langer's book. The Murray report upon examination proves to be an extensive document, in its conclusions and language not unlike Langer's. This

[6] Walter C. Langer, "A Psychological Analysis of Adolph [*sic*] Hitler: His Life and Legend," in Un-classified Historical OSS Records, National Archives, Washington, D.C.

[7] Langer, *Mind of Adolf Hitler*, vi.

[8] *Ibid.* For example: page 155, the last sentence of the second paragraph is not in the original; pages 180–82, the material of the original has been rearranged and much of it omitted; page 206, the last two paragraphs, with their parallels between Hitler's psyche and that of the German people, have been added; page 211, most of section 7, dealing with the eventuality of Hitler's falling into Allied hands, is not in the original. To give a quantitative sample: of sixty-three pages (pp. 130–93) of the book, twenty-nine have major or minor changes to the original.

[9] *Ibid.*, 20–21.

[10] *Ibid.*, 25, 232. Equally unfounded is the subtitle of the book.

[11] Walter C. Langer, "Hitler Source-Book." The "Source-Book" is part of the original Langer report (see note 6).

[12] Langer, *Mind of Adolf Hitler*, 14.

[13] Henry A. Murray, M.D., "Analysis of the Personality of Adolph [*sic*] Hitler, With Predictions of His Future Behavior and Suggestions for Dealing with Him Now and After Germany's Surrender," Oct. 1943. Copy in the President Secretary's File, John Franklin Carter Folder, Franklin D. Roosevelt Library, Hyde Park, N.Y.

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similarity is especially striking in the final sections of the two reports, where the authors speculate on what possible fates might befall Hitler, from natural death to suicide.[14] Since Dr. Murray wrote his paper at the time Langer did and since Langer states that pressure of time prevented him from showing his report to his collaborators, one cannot help wondering to what extent the Langer report is based on Dr. Murray's version. One wishes this relationship had somehow been clarified.

The collaboration between psychology and history, or psychohistory, has been the subject of much debate. I share many of the reservations held by some of my fellow historians about the fusing of these two disparate disciplines.[15] But I also agree with William Langer's view that, in analyzing historical figures, "a good deal can be done to study their character and make their actions more meaningful by gathering all pertinent data and subjecting them to the dispassionate evaluation of qualified persons who have clinical experience to draw upon."[16] I would merely add that such data, besides being pertinent, must also be reliable. Since most historians are not "qualified persons" with "clinical experience," they can hardly pass judgment on the conclusions the author(s) of *The Mind of Adolf Hitler* have drawn from the sources they have consulted. But historians can evaluate the soundness of these sources.

Dr. Langer is much aware of the issue just raised. "The literature [on Hitler]," he says, "although extensive, was mostly unreliable"; and he asks, "How does one screen the wheat from the chaff, fact from fiction, the relevant from the irrelevant, the significant from the insignificant, etc., without a point of reference or orientation?"[17] One obvious answer would be, "One consults a historian." The OSS, after all, employed some of the ablest scholars in this country on Hitler's Germany. But this solution does not seem to have occurred to Langer and his associates. Instead, drawing on the wealth of clinical findings that psychoanalysts have accumulated since Freud's day, they agreed among themselves "on the fundamental nature of the character structure [they] proposed to investigate" (that is, Hitler's). "A survey of the raw material, in conjunction with our knowledge of Hitler's actions as reported in the news, was sufficient to convince us that he was, in all probability, a neurotic psychopath."[18] That hurdle cleared, the rest was easy. "With this diagnosis as a point of orientation, we were able to evaluate the data in terms of probability. Those fragments that

[14] Langer, *Mind of Adolf Hitler,* 209–13; Murray, "Analysis," 221–27. Note also the identical misspelling of Hitler's first name in the original OSS and the Murray reports.

[15] See, for instance, Jacques Barzun, "History: The Muse and Her Doctors," *AHR,* 77 (1972): 36–64.

[16] Langer, *Mind of Adolf Hitler,* vii.

[17] *Ibid.,* 10, 17.

[18] *Ibid.,* 17. In the main part of the book (p. 126), where this assertion is repeated, the text differs from the comparable passage (pp. 127–28) of the original OSS report: "There was *general* [OSS: *unanimous*] agreement among the *collaborators* [OSS: *four psychoanalysts who have studied the material*] that Hitler is *probably a neurotic psychopath* [OSS: *an hysteric*] bordering on schizophrenia [OSS adds: *and not a paranoiac as is so frequently supposed*]." Italics mine.

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could most easily be fitted into this general clinical category were tentatively regarded as possessing a higher degree of probability—as far as reliability and relevance were concerned—than those which seemed alien to the clinical picture."[19] In other words, instead of applying the basic rules of evidence that historians, lawyers, or for that matter any unbiased person would use to get at the truth, the Langer group judged the reliability of their sources by the way they fitted the group's preconceived image of Hitler.[20] This approach, the careful wording of the passage just cited notwithstanding, does not fundamentally differ from what certain Nazi "historians" did when, by carefully selecting a different kind of evidence, they made the Führer into the greatest German ever. It is an imaginative but hardly an exact mode of inquiry.

One of the crucial events in Hitler's life, according to Langer and some other writers, was a primal scene between his parents that little Adolf supposedly witnessed at the age of three.[21] The source for it is a passage in Hitler's *Mein Kampf* describing the unhappy life of an urban worker's family. There is nothing to indicate that the Führer, always most secretive about his early life, intended the passage to be autobiographical, nor does the grim picture painted there agree with what we now know about Hitler's far from dismal childhood.[22] Hitler's clichéd account of lower-class life actually reads not unlike similar such descriptions found in second-rate antiurban novels of the time. But even if one accepts it as (perhaps unconsciously) autobiographical, the question remains whether it describes a primal scene:

> Among the five children there is a boy, let us say, of three. . . . When the parents fight almost daily, *their brutality leaves nothing to the imagination;* then the results of such visual education must slowly but inevitably become apparent *to the little one.* Those who are not familiar with such conditions can hardly imagine the results, especially when the mutual differences express themselves in the form of *brutal attacks* on the part of the father toward the mother or to *assaults* due to drunkenness.[23]

Thus the passage quoted by Langer. A sordid, but hardly a sexual, scene. If we compare the passage with the original German version, moreover, we find that the key expressions (italicized above) lose some of the connotations one might possibly read into them:

[19] *Ibid.*, 17.

[20] Actually the material, which Langer's group interpreted in such a highly selective manner, had already been preselected for them. According to Langer it was gathered by "three psychoanalytically-trained research workers," whose job it was "to comb the literature on file in the New York Public Library and excerpt or abstract those sections that they believed might be pertinent to our project." *Ibid.*, 11.

[21] *Ibid.*, 143, 151. See also Gertrud M. Kurth, "The Jew and Adolf Hitler," *Psychoanalytic Quarterly*, 16 (1947): 28–29; and Norbert Bromberg, "Hitler's Character and Its Development: Further Observations," *American Imago*, 28 (1971): 290–91.

[22] The best book on Hitler's childhood is Bradley F. Smith, *Adolf Hitler: His Family, Childhood and Youth* (Stanford, 1967).

[23] Quoted in Langer, *Mind of Adolf Hitler*, 143. Italics mine.

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Among the five children there is a boy, let us say, of three. . . . When the parents fight almost daily, with an inward rudeness (*innere Roheit*) which leaves nothing to be desired, then the results of such visual education must slowly but inevitably become apparent in the little ones (*bei den Kleinen*). Those who are not familiar with such conditions can hardly imagine the results, especially when the mutual differences express themselves in rude outrages (*rohe Ausschreitungen*) of the father toward the mother or lead to mistreatments (*Misshandlungen*) due to drunkenness.[24]

To interpret this as a description of a sexual attack seems farfetched indeed. Yet this is what Langer does. In discussing Hitler's Oedipus complex, he points to

the *fact* that as a child he *must have* discovered his parents during intercourse. An examination of the *data* [i.e., the passages just quoted] makes this conclusion *almost inescapable,* and from our *knowledge* of his father's character and past history it is *not at all improbable. It would seem* that his feelings on this occasion were very mixed.[25]

The use of exact and assertive terms like *fact, data,* and *knowledge,* balanced by tentative expressions—*must have, almost inescapable, not at all improbable, it would seem*—gives the impression that the author is aware of the uncertain ground on which he finds himself. But such uncertainty does not last long. A few lines further along what had been surmise has become fact: "Being a spectator to this early scene had many repercussions." These are then spelled out.

The example just cited of unwarranted conclusions based on insufficient evidence is unfortunately not unique; it is the rule rather than the exception. Here are additional examples of similarly unfounded assertions:

It is *almost certain* that Adolf had temper tantrums. . . . Their immediate purpose was to get his own way with his mother. . . . *There is reason to suppose* that she frequently condoned behavior of which the father would have disapproved. . . . Life with his mother during these early years *must have been* a veritable Paradise for Adolf. . . .

As he became older and the libidinal attachment to his mother became stronger, both the resentment and fear [of his father] *undoubtedly* increased. Infantile sexual feelings were *probably* quite prominent in this relationship.[26]

It should be noted that while we know a good deal about the outward events of Hitler's early life, we know next to nothing about its more intimate circumstances. But this does not inhibit Langer. Where information is lacking, imagination takes over. A few pages later we learn of another important event in Adolf's early life and of that event's ultimate consequences:

[24] Adolf Hitler, *Mein Kampf* (276th ed.; Munich, 1937), 32–33.
[25] Langer, *Mind of Adolf Hitler,* 151. Italics mine.
[26] *Ibid.,* 150. Italics mine.

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From what we know about his mother's excessive cleanliness and tidiness *we may assume* that she employed rather stringent measures during the toilet training period of her children.

Here, again, *we may assume* that the experience was more intense in Hitler's case than in the average due to the strong attachment and spoiling of his mother in early infancy. Unaccustomed to minor frustrations that most children must learn to endure prior to the toilet training, he was poorly equipped to deal with this experience that plays an important role in the life of all infants. Even now, as an adult, Hitler is unable to cope with frustrating experiences on a mature level.[27]

As Hitler grew older, and especially after he entered the political limelight, information about him became more plentiful until it turned into a veritable flood. But the more material there is, the more difficult it becomes to separate fact from fiction. Between the adulation of his admirers and the abuse of his antagonists, where is the true Hitler? Leafing through the close to a thousand pages of the "Source-Book" that accompanied the original OSS report, one can find evidence to support almost any image of the man—from repulsive, dirty, lazy, and sexually perverted psychopath to attractive, neat, hardworking, and sexually normal statesman. More than ever, therefore, it becomes necessary to scrutinize every bit of evidence before fitting it into Hitler's character structure.

One side of Hitler's personality that always aroused much speculation was his sex life. After examining the mass of contradictory rumors on this subject, Dr. Langer and his collaborators concluded that Hitler was subject to "an extreme form of masochism in which the individual derives sexual gratification from the act of having a woman urinate or defecate on him."[28] As source for this revelation Langer cites Otto Strasser, a prominent Nazi who, after breaking with Hitler in 1930, became one of his most ardent opponents. Strasser obtained his information from Hitler's niece, Geli Raubal, who allegedly spoke from firsthand experience. Elsewhere Langer tells of a different kind of masochistic incident involving a well-known German movie actress, variously referred to as Rene or Renarte Mueller (actually her name was Renate). The authority in this case was her American director, Zeissler, to whom she told her story shortly before committing suicide.[29] What we have, then, are two accounts of different events, both secondhand, one by an enemy of Hitler's, the other by a man who does not even remember the correct name of his alleged informant. Historians would not accept such evidence as valid, especially when there are equally "reliable" accounts of other possible perversions and when Langer himself states earlier that nobody was

[27] *Ibid.*, 163. Italics mine.

[28] *Ibid.*, 134. This version again differs from the original OSS report (p. 138): "He is an extreme masochist who derives sexual pleasure from having a woman squat over him while she urinates or defecates on his face." It should be noted further that Strasser only spoke of urination, not defecation. Langer, "Hitler Source-Book," 919.

[29] Langer, *Mind of Adolf Hitler*, 91, 171.

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really in a position to know about Hitler's sexual activities, except perhaps his personal adjutant. Langer adds, "There are some who believe that his sex life is perfectly normal but restricted."[30] Such a conclusion, however, seems to have been too simple. Instead Langer and his colleagues adopt a "where there's smoke there's fire" approach and accept the perversion that best fits their image of Hitler.[31]

The examples here given, which could be multiplied many times, show that some of the most important conclusions of Langer's book are based on nonexistent, unreliable, or misinterpreted evidence. No matter how plausible the results may seem, they cannot be accepted unless corroborated by more reliable information. In the case of Hitler's family and childhood, much new material not available to the Langer group has already corrected the picture of a drunken and brutal father terrorizing his wife and children.[32] As for Hitler's sexual relations, nothing new has come to light to confirm the account of his masochistic perversion, and from what we know about his relations with Eva Braun (who plays only a minor role in the book) they may have been more nearly normal than assumed. Still, the fact that this question has now been raised so openly and answered so explicitly may lead to further information. There are some less startling statements in Langer's book that since 1943 *have* been confirmed by additional evidence. This holds especially true for the book's early sections on how Hitler saw himself and how others saw him, which contain some useful insights since borne out. If one bears in mind that much of the basic material on Hitler, notably his "Table Talk," was not available to Langer and his colleagues, this part of their work commands considerable respect. To ridicule the book, as has been done,[33] is as uncalled for as it is to claim that it has stood "the test of time."[34] In its as yet unpublished original version the Langer report is an interesting historical document—no less, but also no more.

In the foreword to his brother's book, William Langer tells of an idea he and Walter had "of attempting a collaborative study of some historical figure or movement in which psychological insights could be blended with historical data to yield a deeper understanding of its significance."[35] Unfortunately their scheme never materialized. As this book and subsequent

[30] *Ibid.*, 91.

[31] Another informant on Hitler's sexual aberrations was Ernst ("Putzi") Hanfstaengl (see the anonymous report, "Adolf Hitler, Dec. 3, 1942," in President Roosevelt's Personal File [PPF], 5780), Franklin D. Roosevelt Library, that from internal evidence appears to be by Hanfstaengl). At least one historian, Robert G. L. Waite, accepts Langer's evidence, though "after great hesitation"; see his "Adolf Hitler's Anti-Semitism: A Study in History and Psychoanalysis," in Benjamin B. Wolman, ed., *The Psychoanalytic Interpretation of History* (New York, 1971), 225 n.63.

[32] See Smith, *Adolf Hitler: His Family, Childhood and Youth,* and Franz Jetzinger, *Hitler's Youth* (London, 1958).

[33] See, for instance, the review by H. R. Trevor-Roper in *Book World,* Washington *Post,* Sept. 10, 1972.

[34] Robert G. L. Waite in Langer, *Mind of Adolf Hitler,* 232.

[35] *Ibid.,* vi.

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work on Hitler's psychology has shown, neither psychologists nor historians, working independently of each other, seem to produce mutually satisfactory results.[36] Not only do they lack the necessary training in each others' disciplines, but in their eagerness to make a contribution in a novel and exciting field they often violate the basic rules of evidence.

This latter fact holds true not only for psychologists but also for some of the historians who have tried their hand at psychohistory. An example is Robert Waite, a historian long interested in Hitler's psyche, who contributed an afterword to Langer's book. There he mentions "one critically important fact . . . that Langer did not know anything about. It was only discovered when Russian doctors, who performed an autopsy on Hitler's body in May 1945, found that he was sexually malformed," that is, he had only one testicle.[37] Waite had already mentioned this fact in an earlier article of his, where he quotes from the Russian autopsy report: "In the scrotum, which is singed *but preserved,* only the right testicle was found."[38] In a subsequent article he goes quite a bit further and suggests that Hitler's monorchism (the lack of a testicle) was due to "partial self-castration." "If mutilation had taken place one would normally expect to find evidence of scar tissue on the scrotum. But in this case the whole area was singed *and burned.*"[39] The point here is not Waite's suggestion of Hitler's possible self-castration; such speculation may be necessary and prove fruitful in psychohistory. The point is that in making that suggestion he interprets the statement "singed but preserved" of the Russian autopsy report to mean the same as "singed and burned." This change, be it due to carelessness or a (perhaps unconscious) desire to prove his case, is certainly not sound use of evidence.[40]

It is conceivable that some day there may be scholars equally well versed in both disciplines, history and psychology, to write acceptable psychohistory. But in addition to being trained psychoanalysts they will also need the "clinical experience" that both William and Walter Langer stress as an essential prerequisite. Such persons will be hard to find. Until then, Professor Langer's suggestion of collaboration between scholars from both disciplines remains the most promising approach if psychohistory is to take its place as a respectable field of scholarship.

[36] For examples see the works cited by Kurth, "The Jew and Adolf Hitler"; Bromberg, "Hitler's Character and Its Development: Further Observations"; and Waite, "Adolf Hitler's Anti-Semitism: A Study in History and Psychoanalysis."

[37] Langer, *Mind of Adolf Hitler,* 227.

[38] Waite, "Adolf Hitler's Anti-Semitism," 227. Italics mine.

[39] R. G. L. Waite, "Adolf Hitler's Guilt Feelings: A Problem in History and Psychology," *Journal of Interdisciplinary History,* 1 (1971): 236 n. 33. Italics mine.

[40] Waite's work suffers from many similar instances of inaccurate or imprecise use of evidence. In the article just quoted, the statement (p. 231) that Hitler "took special pains to dictate the precise language of the November Racial Laws of 1935" is not supported by the source cited; nor is the fact that Hitler would "gaze . . . apprehensively" at the blood drawn from him by his doctor; the translation (p. 235) of *Abwässer* as "urine" is a bit free; and one wonders about the source for the assertion (p. 237) that when Hitler "flipped a coin to determine whether he would go on a picnic, heads did not win. Heads invariably lost." There are many other examples.

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# Reviews of Books

## GENERAL

J. H. HEXTER. *The History Primer*. New York: Basic Books. 1971. Pp. xv, 297. $10.00.

J. H. HEXTER. *Doing History*. Bloomington: Indiana University Press. 1971. Pp. viii, 182. Cloth $6.95, paper $2.95.

Tormented by two generations of professional philosophers—the neo-Kantian, neo-Hegelian onslaught of the early twentieth century and the analytical onslaught of mid-century—professional historians are now striking back, sometimes gently, sometimes fiercely. If the philosophers cannot tell us what it means to do history, perhaps we can tell each other. To recent titles on historical thinking by G. R. Elton, Trygve R. Tholfsen, David H. Fischer, Arthur Marwick, Pardon E. Tillinghast, and several others, J. H. Hexter has added *The History Primer* and *Doing History*. The first is a full-dress book, an expanded version in ten chapters of Hexter's article "The Rhetoric of History," in the *International Encyclopedia of the Social Sciences* (1968). The second is a slender bundle of essays on various historiographical themes, including "The Rhetoric of History" itself. Readers of either volume are guaranteed exposure to the same fundamental doctrine about the nature of history.

*The History Primer* "really started, I suppose, at Queens College in 1941, when I heard my then colleague Carl Hempel read his paper, 'The Function of General Laws in History.'" Hexter disagreed with Hempel then and continues to disagree. A host of well-intentioned philosophers have joined Hempel in his attempts to make historians conform to the model of explanation supplied by the natural sciences, and Hexter takes them all on. With the help of such typically Hexterian devices as the Case of the Muddy Pants, the Problem of Galloping Gertie, the Tale of the 1951 National League Pennant Race, and the Parable of the Dumblians, he assures us that "the object of investigation of historians, the human past, is incommensurably different from nature, the object of investigation of natural scientists." History and science use different languages, adhere to different standards of adequacy of explanation, and yield unmistakably different results. As the residents of the village of Dumble were too dumb to realize, there is more than One True Ball Game.

The Ball Game called history stands, so Hexter tells us, somewhere between the sciences and the humanities. It draws on both and may overlap with both. Like the sciences, it often seeks to explain why. Like the humanities, it often seeks humane comprehension of what "what happened" was "like." But history has an identity of its own that defeats all efforts at assimilation by either the sciences or the humanities. Its chief instrument is common sense, it strives for credibility rather than logical rigor, and its mode of discourse is "possessive," whether the historian explains by storytelling or understands by empathetic reconstruction.

*The History Primer* winds up on a modestly evangelical note. The world is besieged by scientific and antiscientific ideologues, "loudmouth peddlers and shills of phony panaceas for the ailments of humanity." Historians "have been perhaps the largest corporate group to resist solicitation" to join their ranks. Historians have resisted because their commitment to common sense, common language, and common humanity runs deep. The very depth of their commitment equips them to offer the



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world the help it really needs. As dispensers of healing charity and human understanding, historians may help save mankind from all those saviors who threaten it with behavioral engineering on the one hand and anarchistic fury on the other.

To call such a book a "primer" was perhaps ill-advised. Although Hexter's message, as he admits, is "conservative, old, and orthodox," he clothes it in a prose so coruscating, convoluted, and cantankerous and bejewels it with so many labyrinthine metaphors both apt and not-so-apt that any beginning student of historiography is likely to perish of brain fever early in the game. Nor does Hexter help him with anything like a full exposition or intellectual history of the ideas he is attacking. There is no bibliography; not even an index. I particularly regret that Hexter did not map out clearly his areas of agreement with such antipositivist pre-Hempelian philosophers as Wilhelm Dilthey.

But never mind. The whole tiresome business of analytical philosophy is shaken down to its proper size in both of these books, and Hexter goes a good day's journey toward providing us with a definition of the autonomy of history based on the autonomy of its rhetoric. The medium is not the message—or at least it does not exhaust the message—but the organic relationship of the two is well worth noting.

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MELVIN SMALL, editor. *Public Opinion and Historians: Interdisciplinary Perspectives.* Detroit: Wayne State University Press. 1970. Pp. 199. $7.95.

*Public Opinion and Historians* contains papers read at a symposium at Wayne State University in May 1967. The symposium was arranged by the editor of the volume, historian Melvin Small, who writes that he was convinced that scholars in fields other than history had much to contribute to the study of public opinion. While "most historians [were] unaware of or uninterested in [this] relevant research in sister disciplines" (p. 9) Small sought to combat that situation through the symposium, and the symposium's plan is reflected in the book—a paper concerning some aspect of the study of public opinion by a scholar in a field other than history is followed by a comment on the paper by a historian. The five papers and five comments are introduced through an additional paper by Small on "Historians Look At Public Opinion," with comment by a political scientist, J. David Singer.

This book is superior to many interdisciplinary colloquia in that it is focused on a specific topic, public opinion, and also that it is broader in concept, including papers both by behavioral scientists and, in addition, by a professor in an English department who describes his research methods as "humanistic." Because of its broad concept, the volume can be described as essentially two dialogues—one in which behavioral scientists talk and historians respond, concerning the study of public opinion with respect to foreign policy, and a second dialogue in which "humanistic" scholars talk about the study of "images" and "culture," with comments by historians.

In the first dialogue, which makes up most of the book, Small and several behavioral scientists urge historians to be more precise and more systematic in studying public opinion: more precise and systematic in defining "opinion" and "images," in carefully distinguishing between "editorial opinion" and "public opinion," and in distinguishing between the opinion of "elites" and the opinion of the "mass public." Political scientist Bernard C. Cohen, in one of the more interesting papers in the volume, implies that imprecise and unsystematic methods can lead to inaccurate or unproven substantive conclusions. He notes, for example, the numerous assertions by historians and "traditional" political scientists that "public opinion" was crucial in shaping or in setting limits to decisions in United States foreign policy. By contrast, he describes from his own studies "a foreign policy making establishment that is under very few public opinion constraints" (p. 70), and he suggests that United States presidents, rather than being restricted by "public opinion," are powerful shapers of "public opinion." He calls for "systematic and focused attention, in historical research" upon what makers of foreign policy decisions (presidents, secretaries of state, congressmen, and others) perceived "public opinion" to be, and upon "what specific modifications or con-

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straints were *thereby* imposed on their foreign policy preferences or intentions" (p. 79). Another political scientist, Richard L. Merritt, provides an example of one method used by behavioral scientists to study "public opinion," the "content analysis" of six German high school history textbooks. Sociologist Paul F. Lazarsfeld, not restricting himself solely to studies of public opinion, describes some writings by behavioral scientists that he considers useful for historians, and some writings by historians that he considers useful for sociologists.

The historians who comment upon these papers by the behavioral scientists express some interest and some reservations. Bradford Perkins, commenting on Lazarsfeld's paper, says that historians should search more broadly in other disciplines for approaches, insights, and methods, but that they should remain cautious and skeptical. Alfred H. Kelly states that he has considerable sympathy with "large portions" of Cohen's paper, but he questions whether the impact of "public opinion" on foreign policy should be denigrated as much as Cohen suggests. John Higham writes that he found himself "simultaneously admiring the elegance and apparent economy" of Merritt's content analysis, while at the same time "worrying about the price he [Merritt] has paid" (p. 176) in limiting himself to a small range of data and in emphasizing method over substance.

In the book's second dialogue, between "humanist" scholars and historians, Leo Marx, from an English department and an American Studies program, presents one of the clearest and most provocative papers in the symposium. He contrasts the aim and methods of behavioral scientists or empirical historians in describing "public opinion," with the goal and techniques of the "humanist" scholar in investigating the "culture" of a group—and in particular the "high culture" reflected in works of imaginative literature, which may have had few readers and virtually no influence on "public opinion" at the time of their publication. Political scientist Harold R. Issacs declares that both social scientists and humanists can view external reality only through the "prisms" of their own minds, with the distinctive "images" or "pictures" that result from the "prisms." He urges more study of the pictures or images in the minds of individuals, especially individuals who were decision makers in foreign policy, images that might come from novels, textbooks, chance personal experiences, or a variety of other sources.

Historian Athan Theoharis finds in Issacs's paper valuable suggestions for historians but criticizes the failure of Issacs "to document or critically assess the factors contributing to image formation and the nature of the relationship of image to policy making and public opinion" (p. 109). Historian Edward Lurie praises little in Marx's paper but instead describes it as "high-cult" in attitude, with affirmations that are "anti-intellectual and contemptuous of the lower orders of society" (p. 131).

After wrestling with discussions of method by scholars in fields other than history, it is undoubtedly refreshing to most historians to hear the clear, direct, uncomplicated voice of common sense. In this case the voice belongs to Satchel Paige, as quoted by Lurie, when Paige described his "method," which allowed him to continue pitching a baseball so well despite his advanced age: "Man, I just stays loose." Paige's wisdom, as always, is impressive, but what constitutes "staying loose" for historians? Many historians can agree that "staying loose" means not getting locked-in to esoteric behavioral science or to esoteric "high-cult" humanism, but does it also mean not getting locked-in to imprecise, unsystematic, and unimaginative methods of research and analysis, even if they are widely accepted in the guild of historians? To this last question Small and some of the other historians in this book would seem to answer "yes," and so would I.

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NATHAN G. HALE, JR., edited with an introductory essay by. *James Jackson Putnam and Psychoanalysis: Letters between Putnam and Sigmund Freud, Ernest Jones, William James, Sandor Ferenczi, and Morton Prince, 1877–1917.* Translations of German texts by JUDITH BERNAYS HELLER. (A Commonwealth Fund Book.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xiv, 384. $14.00.

In James Burnham's exhaustive work on the spread of Freud's concepts in the United States, Burnham termed James Jackson Put-

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nam the country's "most interesting convert to psychoanalysis." It is interesting, indeed, that this outstanding neurologist should have supported such a movement; his philosophy, solidly rooted in Unitarianism, Transcendentalism, and American Hegelianism and influenced by his contemporaries Bergson, Royce, and William James, would seem completely antithetical to Freud's ideas. Putnam became convinced, however, that some important psychoanalytic theories were valid, and, moreover, he was much impressed with Freud during the latter's famous visit to America in 1909. Finally, it was Putnam's propensity to champion the cause of the underdog, and in 1909 Puritanism was still sufficiently strong in the American medical profession to pose a bulwark against Freud's emphasis on sex. This proved a challenge to Putnam, an influential member, as Mr. Hale emphasizes, of "New England's professional aristocracy." It was Putnam who "chiefly secured for Freud's views a serious public hearing."

These facts, discussed in the introduction, are already known, and except for details on the formation of the American Psychoanalytic Association, the letters offer nothing especially new. Yet it is fascinating to find those facts highlighted, particularly in Putnam's correspondence with Freud and Ernest Jones, but with William James, Sandor Ferenczi, and Morton Prince as well. There are also highly interesting sidelights. The letters show, for example, that despite Putnam's staunch loyalty to Freud at a time when Adler, Jung, and Stekel were abandoning him, the American persistently pressed his own divergent convictions, albeit always in modest fashion, emphasizing Freud's role as master and Putnam's own as student. Freud and Jones, on their part, reveal themselves as fully cognizant of Putnam's importance by their careful responses to concepts that must sometimes have appalled them. Freud wrote that "you make psychoanalysis seem so much nobler and more beautiful: in her Sunday clothes I scarcely recognize the servant who performs my household tasks." Yet it was Putnam's thoughts on religion that prompted Freud, as he told Putnam, to reading that would later result in the publication of *Totem and Taboo* (London, 1919). And Jones believed, Hale points out, that Putnam's interest in the "nobler" aspects of man's development—conscience, philosophy, and religion—led Freud to studies culminating in "The Ego and the Id." Hale comments, moreover, that various problems stressed in Putnam's letters would form the bases for later conclusions of ego, existential, and Gestalt psychology.

An outstanding characteristic of Putnam as a pioneer in the American psychoanalytic movement was what Jones termed Putnam's "unity complex." This appeared clearly in the exchange among him, Jones, and Prince, with Putnam as self-styled "buffer." (Incidentally, one of the most interesting of the letters is Prince's emotional expression of despair to his friend over what he saw as the increasingly cultish aspects of a movement that he had earlier promoted.) The tendency of psychoanalysts to attack adherents of their own as well as of other psychiatric doctrines aroused Putnam's anxiety; he asked Freud why analysis did not make them more mature.

A source of frustration to the reader is the fact that many of the letters are no longer extant; he is thereby frequently deprived of the concurrence or clash of views. Hale's explanatory footnotes at the close of each letter as well as his introduction are helpful in providing background, but familiarity with the studies of Burnham and Hale on the early period of American psychoanalysis, in addition to Jones's work on Freud, would render the letters even more meaningful. The collection is followed by a chronology of events and then of the letters, a selective bibliography, a brief section of bibliographical notes on the principal figures mentioned, and, finally, a repetition of Freud's letters in their original German. It is a carefully edited collection.

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MAX SCHUR. *Freud: Living and Dying.* New York: International Universities Press. 1972. Pp. xiii, 587. $20.00.

Here before us lies a great load of stuff meant to represent the structure of Freud's life. Although the aim may be great, although the stuff may be weighty, it cannot turn into the structure: it must be turned, changed, made to

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be the structure. Schur has not fulfilled his aim.

Schur died in 1969 after finishing the manuscript for this book, but before its final editing. A specialist in internal medicine, he was Freud's physician from 1928 until the master's death in 1939, and after settling in the United States Schur himself became a psychoanalyst.

Schur cherished a double intent: to record forever his memories of Freud's heroic struggle against the cancer in his jaw, first detected in 1923, and to show how Freud's courage grew from the deepest substance of his mind and character. Ernest Jones, in his biography of Freud and with help from Schur, had told the tale of Freud's suffering. The harrowing catalog of papillomas, leucoplakias, excisions, hemorrhagings, fetid odors, and struggles with the prosthesis called "the monster" does not fail to horrify again in a somewhat fuller retelling.

Freud's conduct, as well, does not fail to draw forth sympathy and admiration, but we do not understand the inner nature of his achievement very much more adequately than we did in 1957, when Jones's final volume appeared. True to his commitment as a psychoanalyst, Schur wanted to link Freud's final years with all the decades of growth that came before. Ultimately the task baffled Schur, and he settled for strings of quotations on life and death from Freud's major works. Schur also quotes extensively from unpublished portions of Freud's extraordinarily revealing correspondence with Wilhelm Fliess, Freud's intimate friend during the years that saw the creation of psychoanalysis. Schur strung his material together with much laudatory verbiage and not nearly enough psychological probing. Thus he discusses at great length, but without a final resolving assessment, Freud's lifelong preoccupation with "critical" years, dates at which he thought he would die. Thus Schur discusses Freud's "uncanny" intuitions about the role of unconscious mental factors in the causation of bodily disease, but again, with no decisive verdict on the connection of such views to the appearance of Freud's own illness. Thus Schur takes up Freud's attitude toward religion and repeats the standard pieties about his rationalism, in the face of Freud's addiction to number mysticism and to peopling his study with divinities from pagan antiquity. Thus Schur raises the question of the role of Jewish tradition in Freud's life, but Schur cannot bring himself to a final judgment on that role.

Schur's memories deserve to be recorded, and his attempt to tie Freud's death to his life furnishes many valuable hints that could lead to a more satisfactory solution of the problem. Who knows how long it will be until we have a really good biography of Freud, resolutely historical and deftly psychoanalytic, one that dissects out main threads and then tangles and untangles them as they reach onward in time. There appears to be no rule or guiding pattern that forecasts the appearance of such a biography. Marx still lies swathed in platitudes and inexactnesses. More light flickers around Darwin, perhaps foretelling the nearness of a paradigmatic interpretation, perhaps only enabling us to see more clearly how sizable the real problems are. Historians should draw insight and incitement from the aim and the failure of Max Schur's big book.

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LEON Ó BROIN. *Fenian Fever: An Anglo-American Dilemma.* New York: New York University Press. 1971. Pp. x, 264. $7.95.

In the past few years Mr. Ó Broin has established himself as one of the most respected writers on modern Irish history. His *Dublin Castle and the 1916 Rising* appeared in 1966, followed in 1969 by *The Chief Secretary: Augustine Birrell in Ireland.* Now, as a prologue to these volumes, he has written a study of the early years of Fenianism, the "recurring fever" that revived the republican ideal and eventually, after much reorganization, played a primary role in the Easter Rising.

*Fenian Fever* is a study in frustration as the author reconstructs the failure of Fenianism to achieve its goals during the 1860s. Despite clever organization and sound basic strategy, the bitter, divisive quarrels of the leaders, the surprising number of informers produced from the movement, the apparent ease with which police and military "spies" monitored Fenian activity in Ireland, England, and the United States, all conspired to deny the hard-core membership the successful republican revolution they so earnestly desired. A rising of sorts

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was attempted in 1867 but was handled with relative ease by those in authority. The men who in effect governed Ireland showed considerable skill in treating the symptoms of the "fever," and Mr. Ó Broin is at his best when probing their activities. When confronted with the causes of discontent, the Castle hierarchy of the 1860s seem to have been only marginally more enlightened than many of their predecessors. But in dealing with the legacies of discontent the leadership apparently benefited from the mistakes of earlier administrations. As the lord lieutenant observed, "the recollections and the heart burnings consequent on the suppression of rebellion by warlike means, was a more enduring and greater certainty than the rebellion itself" (p. 105). Thus Castle policy, as set forth in late 1866, was the prevention of rebellion rather than its suppression by armed force. And the following year the ease with which the attempted rising was contained and broken by the authorities was due more to the "preventive" contributions of the informer and the spy than to the military efforts of the policeman and the soldier.

Specialists in American history may accuse the author of abandoning Fenianism in the United States about midway in the book, but the reader interested in Anglo-Irish relations will surely agree that Mr. Ó Broin has written an excellent book on an involved and difficult topic. *Fenian Fever* comes close to the level of Mr. Ó Broin's *Dublin Castle,* and that is a very high level indeed.

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EDWARD H. SIMS. *Fighter Tactics and Strategy, 1914–1970.* Forewords by JOHN C. MEYER and J. E. JOHNSON. New York: Harper and Row. 1972. Pp. xvi, 266. $6.95.

RICHARD C. LUKAS. *Eagles East: The Army Air Forces and the Soviet Union, 1941–1945.* Tallahassee: Florida State University Press. 1970. Pp. ix, 256. $10.00.

Some fifty years ago Theodore Roosevelt wrote, "The ordinary air fighter is an extraordinary man; an extraordinary air fighter stands as one in a million among his fellows." In tracing the development of tactics and strategy of aerial combat through the two world wars, Korea, and Vietnam Edward Sims has concentrated on the factors that have produced the extraordinary fighter pilot described by Roosevelt as "one in a million." Even though the flying characteristics of a fighter plane (whether it be a Camel, Fokker, Mustang, Messerschmitt, or Phantom jet) dictate fighting techniques to a substantial degree, the great aces such as Richthofen, Rickenbacker, Luke, Hartmann, and Gabreski have found several common tactical answers. According to the author the most successful offensive tactics in fighter versus fighter combat have included "surprise," "close in shooting," "a fast pass from altitude," and "two elements of mental judgment: the ability to size up quickly and accurately conditions as they are encountered, and prudence." Of course all great fighter pilots have demonstrated the hunter instinct.

Sims, himself a World War II fighter pilot, is convinced that competitive fighters flown by competent pilots using sound tactics and strategy are the essential ingredient of aerial, and frequently land and sea, success. Few would quarrel with one of his examples—the 1967 Arab-Israeli war. In the opening hours of that conflict Israeli Air Force fighters virtually wiped out the enemy air force and from that point Israeli ground forces were unstoppable.

In developing his clues to fighter success, Sims has relied heavily upon the diaries and memoirs of outstanding flyers such as Adolf Galland, Billy Bishop, and James McCudden. Unfortunately he uses few footnotes. Nonetheless I wish *Fighter Tactics and Strategy* had been available in 1942.

*Eagles East* is an excellent example of military-diplomatic history. Making thorough use of archival material, much of it exploited for the first time, Richard C. Lukas tells the story of the air force aspects of Russo-American relations during World War II and the difficulties which made that relationship a "dirge of frustration and disappointment." He discusses in careful and somewhat laborious detail such problems as Soviet suspicion and distrust over allocation of military aircraft (under the Lend-Lease Act both Russia and Britain were competing with the American forces for the few planes being built in the United States in 1941–42), the types of aircraft, and the delivery of equipment via the Fairbanks-Nome (Alsib),

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Abadan (Iran), and North Russia (Archangel) routes. In addition the author delves into the controversies arising out of American efforts to secure Soviet participation in the Pacific War and her attempt to base an air force on Russian territory (Project VELVET). Even American shuttle-bomb raids between England and Italy to Soviet bases suffered from poor liaison. Perhaps only the diplomatic persistence of President Franklin Roosevelt held the precarious alliance together. In the end, however, the United States delivered nearly 15,000 aircraft to the Soviets. These planes—mostly P-39, P-63, and P-40 fighters, A-20 light bombers, B-25 medium bombers, and C-47 cargo planes—played a significant role in the Russian victory on the Eastern Front.

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CLARENCE G. LASBY. *Project Paperclip: German Scientists and the Cold War.* New York: Atheneum. 1971. Pp. x, 338. $8.95.

"Project Paperclip" was the code name referring to the American Armed Forces program to bring German scientists and engineers to the United States in the years following World War II. The most famous group of experts—the V2 rocket engineers centering on Wernher van Braun—and their accomplishments have become well known to the American public, but the historical evolution and changing rationale for the policy have been obscure. To cut away the many rumors and erroneous impressions that have had a long life as the cold war ran its course, Clarence G. Lasby has written and secured the publication of a study that brings a flood of light by the use of scholarly judgment and a broad perspective.

As the allied armies moved into Germany in 1945, a number of teams of technical investigators accompanied them. Short on policy direction from above and long on competitive spirit against not only the Russians but also the French, British, and the other American services, the teams rounded up as many documents from the German research effort as they could find. Almost immediately the officers in the field came to recognize, however, that the real intellectual booty to be had from Germany was in the minds of the technical people and that full exploitation of them required attention to their personal and professional futures as well as to their pasts. Lasby then traces the fortunes of the project between the poles of antinazism and anticommunism, the mopping up of World War II and the girding on for the cold war, the desire to "exploit" the Germans and the desire to "deny" them to the Soviets, the desire to destroy Germany's scientific supremacy and the recognition that military research had become a key constituent of power.

In balancing these themes Lasby has made a deep study of the way in which the United States government makes policy—the ironies, the paradoxes, the pluralism, the maddening crosscurrents that swirled around a small but exceptional group of human beings. The Department of State appears more a villain than the military services. There emerges a certain formal resemblance between the intellectual migration in the late 1940s and that of the refugees of the 1930s, some of whom became public opponents of Paperclip. The postwar Germans, however, came in spite of their politics and on individual contracts as wards of the military departments. Their predecessors had come into American life largely by way of universities and only later worked for the government.

An additional saga of policy making appears in Lasby's preface where he recounts his difficulties in getting access to documents and clearance for publication. This volume is an indication of what has to be done if the huge gap between official history and journalism is to be filled by independent scholars with high professional standards working from full documentation.

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## ANCIENT

ERNST POSNER. *Archives in the Ancient World.* Cambridge, Mass.: Harvard University Press. 1972. Pp. xvii, 283. $10.00.

Ernst Posner, whose distinguished career as an archivist spans two continents and six decades and involves administration, research, and teaching, has now turned his attention to the ancient world. The result is a well written, carefully researched study, the first general survey of archival techniques and institutions cov-

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ering all of antiquity, from the civilizations of the Near East beginning with Sumer, through Pharaonic, Ptolemaic, and Roman Egypt, Greece, and Rome, to the Neo-Parthian Empire. The utility of such a study, in a field where source criticism holds an honored place, will be obvious.

For each period Posner discusses the physical arrangements for the preservation of records, the administrative machinery responsible for the archives, the nature and use of the records, and the degree to which the archival institutions reflect the civilizations that produced them. The nature of the evidence, of course, does not permit a complete discussion of every aspect for each civilization, and Posner's chapters therefore vary in content in accordance with the material available.

In treating the clay tablet archives, for example, Posner is able to draw on the vast numbers of surviving tablets and on archeological evidence for their storage, but in dealing with Greece and Rome, where little genuine archival material survives (Posner distinguishes inscriptions from the original records), he rightly concentrates on administrative procedures, personnel, agencies, and the two archives buildings, the *Metroon* and *tabularium*.

The great value of the book is that it gives us, in concise and accessible form, a survey of all ancient archives. This is also, of necessity, its major limitation: the book is an introductory survey, not a detailed study, as Posner himself states (p. 11). Further, the field is vast, and Posner notes that, in general, he has had to rely on secondary materials rather than on the original sources (p. viii). As a result the historian will find certain peculiarities and limitations: a few disconcertingly brief summaries, as when two millenia of Near Eastern history are condensed to one page (p. 25); citations of, for example, Mommsen rather than the original passage in Cicero (p. 165 n.10); a few mistakes, made inevitable by the vast period covered: Caesar, for example, might smile to find that Catulus was elected *pontifex maximus* after all (p. 172). One final desideratum: it would clearly be instructive to have Posner, an expert archivist, assess the nature and extent of archival material in, for example, Suetonius or analyze the accumulations of material from, say, Mari. Would it be ungrateful to ask for further studies of ancient archives, perhaps in greater detail?

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E. A. LOWE. *Palaeographical Papers, 1907–1965*. In two volumes. Edited by LUDWIG BIELER. New York: Oxford University Press. 1972. Pp. xviii, 347, 60 plates; pp. x, 350–645, plates 61–150. $55.00 the set.

"Unlike poets, palaeographers are made, not born," Lowe was wont to say, alluding to the concatenation of unforeseeable circumstances that made him a pupil of Ludwig Traube and later brought him to an academic post in which he could devote himself to scholarship without wasting much time on the sterile routine of diploma factories. He could have added that he had the good fortune to reach maturity in an age of heady optimism, when men could have implicit faith in learning and in the future of a civilization that had not yet committed suicide. But Lowe's modest dictum could be reversed with equal truth. He was born with an extraordinary acuity of eyesight that even age did not irreparably impair; with an ability to recognize the minute or seemingly inconsequential details that, as Sherlock Holmes so often remarked, all men see but few observe; with a capacious and vigilant memory that never failed to remind him of minutiae observed years before; and with the rare talent, paradoxically called common sense, that enables some men to ride, with tight rein and bit, the wild horses of human imagination. No mere concatenation of accidents made the man who, by common consent for a half century, was the foremost living paleographer.

These handsome volumes reprint in chronological order the major articles that Lowe contributed to periodicals, collaborative undertakings, and *Festschriften*, including three tabulations that are simply indispensable for critical study of classical Latin texts. With the exception of some superseded appendixes the articles are reprinted verbatim; many of the accompanying plates, made from new photographs, have been greatly improved in clarity.

Articles published in various collections over so long a time necessarily contain a considera-

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ble amount of repetition and some passages that Lowe would certainly have revised for a second publication. He discussed the possible precedents for the use of a tall letter "i" (*I-longa*) in the Visigothic and Beneventan scripts in an article published about a half century before the use of that symbol in the first century of our era was studied and elucidated by epigraphers. Considering (p. 249) an odd jotting in a blank column of a manuscript that he conjecturally associated with Würzburg and Fulda, he assumed that the medieval name "Gamundia" (or "-ium") must designate modern Hornberg, a comparatively remote town; he evidently did not know that Gamundia was also the name of modern Gemünden, situated above Würzburg at the confluence of the Main and the Saale and on the direct route to Fulda, but the toponymy was confused in the reference works at his disposal. Even in the new three-volume edition of Plechl's revision of Graesse's *Orbis Latinus* (1972) this "Gamundia" is, by a strange lapse, entered as a variant under "Ioviacum Noricorum."

The two examples suggest what I am sure a complete list of such passages would show, that where Lowe's statements require modification, he was relying on information from reference works and similar sources, while in strictly paleographic matters his eye was never at fault. His acute study of the famous Second Medicean manuscript of Tacitus is not in the least impaired by his acceptance of the then unquestioned dictum that it was the archetype of all other extant manuscripts—an assumption that Bieler, by a rare intervention, corrects in his bibliography of Lowe's writings (p. 602), but Bieler's note must now be itself corrected in the light of the collations by Ingeborg Schinzel that appeared late last year in supplement 3 of the *Wiener Studien*.

These articles are both an introduction and a supplement to the twelve volumes of Lowe's monumental *Codices Latini antiquiores*. He confined himself strictly to an intricate and technical discipline, but his work could serve as a model for all study of the past. Lowe never forgot "to draw a sharp distinction between fact and conjecture" (p. 181) and to exclude conjectures from conclusions. As the distinguished editor of Cicero's letters, Hacan Sjögren, observed "est nesciendi quaedam ars"—the art of recognizing the precise limit to which our evidence will permit us to go, and of resigning ourselves to the bitter fact that what lies beyond that limit is unknowable and cannot be recovered by ingenious conjectures and alembicated arguments that are more akin to "science fiction" than to scholarship.

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J. W. WEVERS and D. B. REDFORD, editors. *Studies on the Ancient Palestinian World, Presented to Professor F. V. Winnett on the Occasion of His Retirement, 1 July 1971*. (Toronto Semitic Texts and Studies, Number 2.) [Toronto:] University of Toronto Press. 1972. Pp. x, 171. $10.00.

Since *Festschriften* are notorious hiding places, it is probably worthwhile to list the contents of this one.

Pp. 3–17: James B. Pritchard, "An Incense Burner from Tell es-Sa'idiyeh, Jordan Valley." A limestone block, fourth century B.C., dubious inscription ("Of Zakkur"?), geometric decoration and crude graffiti of man and horse added later. A review of similar objects shows the closest parallels are Babylonian, but the decorative motifs are so simple and the objects so few that this may be chance.

Pp. 18–28: William L. Reed, "The Archaeological History of Elealeh in Moab." Identification of the site is uncertain, and nothing of any importance has been found there.

Pp. 29–33: A. D. Tushingham, "Three Byzantine Tombstones from Dhiban, Jordan." All three show a square cross inscribed in a circle above an inscription reading "Stela of X son of Y, (aged) Z years." The names are mostly uncertain and all insignificant.

Pp. 34–45: E. J. Revell, "The Placing of the Accent Signs in Biblical Texts with Palestinian Pointing," argues that these signs probably generally marked stressed syllables (their use is not consistent because the pronunciation was changing).

Pp. 46–74: John W. Wevers, "The Textual Affinities of the Arabic Genesis of Bib. Nat. Arab. 9." The text (of a thirteenth-century manuscript) is an example of a translation made from the Septuagint, much contaminated by the influences of the Coptic, Chrysostom, and Theodoret.

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Pp. 75–85: Ronald J. Williams, "Energic Verbal Forms in Hebrew"—forms with an added "n." All cases in Old Testament Hebrew and selected cases from half a dozen other Semitic languages are reviewed to show that these have no significance. How true.

Pp. 86–101: W. S. McCullough, "Israel's Eschatology from Amos to Daniel," undertakes to prove that "all the really important features in this eschatology, at least down to Daniel, can be most satisfactorily accounted for by carefully scrutinizing Israel's own traditions." No doubt they can—to McCullough's satisfaction. He is satisfied if he can find any trace in early material of what is widespread later (why it became widespread is never asked)—and his dating of dubious material is unusually convenient for his thesis. Generally dogmatic and occasionally ignorant.

Pp. 102–16: Robert C. Culley, "Oral Tradition and Historicity," shows conclusively that, while oral tradition often distorts historical events, it frequently preserves historical elements. Fancy that!

Pp. 117–40: N. E. Wagner, "Abraham and David," a penetrating and original discussion of the problem of dating the J material about Abraham. The notion that J saw the Davidic kingdom and covenant as the fulfillment of the promises and renewal of the covenant given to Abraham is demolished. By far the most thoughtful and thought-provoking thing in the volume, this deserved more prominent publication.

Pp. 141–56: D. B. Redford, "Studies in Relations between Palestine and Egypt during the First Millennium B.C. I. The Taxation System of Solomon." Disproves the notions that the titles of Solomon's officials and the types of his architecture came from Egypt, but then shows that his method of taxation—dividing his territory into twelve districts and requiring each to furnish provisions for one month of the year—probably was Egyptian. I hope the promise implicit in the title will be carried out (see Redford's important book on the Egyptian background of the Joseph story).

Pp. 157–68: A. K. Grayson, "Cylinder C of Sîn-šarra-iškun, a New Text from Baghdad." Sîn-šarra-iškun was one of the last kings of Assyria; his reign ended with the fall of Nineveh in 612. This cylinder, of which a new piece now gives us an almost complete text, reports only that his majesty, in his accession year (unknown), reconstructed a building of alabaster erected by Sennacherib as a western entrance to the royal palace of Nineveh. Dates, titulature, patron deities, concluding curses.

Pp. 169–71: A. Bembenek, "Bibliography of Professor Winnett's Publications." Mainly on early Arabic material and Jordanian archeology. Convenient.

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BENJAMIN MAZAR, editor. *The World History of the Jewish People.* First Series, *Ancient Times (Patriarchs and Judges).* Volume 2, *Patriarchs;* volume 3, *Judges.* [New Brunswick:] Rutgers University Press. 1970; 1971. Pp. 306; viii, 366. $20.00 each.

The Israel Jewish History Publications Ltd. is issuing the English version of *The World History of the Jewish People* through Rutgers University Press. This review concerns volume 2 (*Patriarchs,* 1970) and volume 3 (*Judges,* 1971), both edited by Benjamin Mazar.

The authors of the various chapters submitted their manuscripts about twenty years ago. But around four years ago they were given a chance to make some changes while reading proof. Accordingly, the volumes before us represent in essence the scholarship of the mid-fifties somewhat corrected and updated.

During the two decades between planning and publication, the editor and his staff modified the structure of the work as a whole. For example, the reviewer's chapter entitled "Cultural and Religious Life" (vol. 3, ch. 4) was written to cover New Kingdom times or roughly the second half of the second millennium B.C. It therefore deals with the Patriarchal age as well as the period of the Judges, though it is printed in the *Judges* volume. Many chapters in volumes 2 and 3 are applicable to both volumes, for they were written before the original concept of "New Kingdom Times" was split into "Patriarchs" and "Judges," apparently for the sake of using familiar Old Testament terminology.

Benjamin Mazar, the leading contributor, has written four chapters: "Canaan in the Patriarchal Age" (vol. 2, ch. 9), "The Historical Development" (vol. 3, ch. 1), "The Exodus and

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the Conquest" (vol. 3, ch. 5), and "The Philistines and Their Wars with Israel" (vol. 3, ch. 8). On page 166 of the latter, Mazar suggests that the Philistines became Semitized linguistically after coming to Palestine from Crete. H. L. Ginsberg's "The Northwest Semitic Languages" (vol. 2, ch. 6) incidentally recognizes that the Philistines spoke a Semitic language that they adopted as a "non-Semitic Sea People" ruling Canaanite masses (pp. 110–11). These two volumes are the first history in which there are authors who realize that the Philistines spoke Northwest Semitic and not some Indo-European or unidentified language. Since these chapters of this history were written it has become clear that the Philistines brought their Northwest Semitic speech with them from Crete, as I was able to add in proof (p. 303 n.8).

The late E. A. Speiser contributed three chapters: "Historiography and Historical Sources in Ancient Mesopotamia" (vol. 2, ch. 1), "The Patriarchs and Their Social Background" (vol. 2, ch. 8), and "The Manner of the King" (vol. 3, ch. 13).

M. Greenberg writes on the "Hab/piru and Hebrews" (vol. 2, ch. 10), a hardy perennial discussed also by a number of other contributors to these volumes.

J. Wiener's "The Religious Culture of the Jewish People in Its Beginnings: The Faith and the Cult" (vol. 3, ch. 10) parallels much of M. Haran's "The Religion of the Patriarchs: Beliefs and Practices" (vol. 2, ch. 12). Like a number of other contributors to these volumes they retain much of the classical structure of nineteenth-century higher criticism. The problems of criticism are treated comprehensively in "The Bible and its Historical Sources" (vol. 2, ch. 4) by J. Liver who also writes on "The Israelite Tribes" (vol. 3, ch. 9).

S. E. Loewenstamm discusses "Ugaritic Writings" (vol. 2, ch. 2) and "Law" (vol. 3, ch. 2).

S. Yeivin covers "The Patriarchs in the Land of Canaan" (vol. 2, ch. 11) as well as "The Canaanite Inscriptions and the Alphabet" (vol. 2, ch. 3). The subject of the alphabet merits a fresh treatment in the light of developments during the last few years. For one thing, it has become clear that there is some substance to an old discarded view: that the letters of the alphabet go back to zodiacal signs. Obviously this cannot be limited to the twelve signs of the solar zodiac; we must reckon also with lunar zodiacs connected with the days of the month. "Aleph" is not merely "ox" but Taurus; "mem" (water) is to be compared with Aquarius; "nun" and "samek" both meaning "fish," with Pisces; "tau" (cross) with the constellation Crux; and so on. The Ugaritic alphabet is of special interest because it has twenty-nine distinctive letters, plus a phonetically unnecessary thirtieth letter that duplicates the nineteenth ("s") in sound. A lunar month, extending from new moon to new moon, is always more than 29 but less than 30 days, so that in practice, a month in a lunar calendar has either 29 or 30 days. The Ugaritic alphabet, with either 29 or 30 letters, corresponds to the requirements of a lunar calendar.

The critical area for the transmission of the alphabet from Northwest Semites to the Greek was Crete. It was there that the same alphabet was used to write both Eteocretan and Greek in bilingual communities. Eteocretan is Northwest Semitic with many features that go with Aramaic. This ties in with the Greek letter-names ending in the Aramaic postpositive article "-a": "alpha, beta, gamma, delta, iota, kappa, lambda," etc., as opposed to Phoenician "aleph, bet, gimel, dalet, yod, kaph, lamed," etc.

H. Tadmor analyzes the "Chronology of the Ancient Near East in the Second Millennium B.C.E." (vol. 2, ch. 5); Y. Yadin, "Warfare in the Second Millennium B.C.E." (vol. 2, ch. 7); I. Mendelsohn, "Society and Economic Conditions" (vol. 3, ch. 3); Y. Aharoni, "The Settlement of Canaan" (vol. 3, ch. 6); and H. M. Orlinsky, "The Seer-Priest" (vol. 3, ch. 12).

For the historian the volumes under review will recall the fascicles of the *Cambridge Ancient History*, rather than Eduard Meyer's *Geschichte des Altertums*. Unlike Meyer's consistent opus, which emanated from one mind and one pen, the volumes before us contain chapters written by different scholars with different strengths and different points of view. The editor could not, and did not try to, impose intellectual uniformity on the contributors. Moreover, the chapters do not constitute a tightly conceived whole, within which one chapter neatly follows the other, without duplication or lacunae. Instead of a history, we have a collection of valuable essays concerning Ca-

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naan and associated areas of the Near East in the second millennium B.C., particularly in the Late Bronze and First Early Iron Ages.

The volumes are richly illustrated with black-and-white photographs, line drawings, and charts.

Every historian of ancient Israel and of the Near East will want to own these volumes and refer to them constantly.

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F. E. WINTER. *Greek Fortifications. (Phoenix* Supplementary Volume 9.) [Toronto:] University of Toronto Press. 1971. Pp. xviii, 370. $15.00.

The most numerous, conspicuous, and often esthetically pleasing monuments that have survived from Greek antiquity are its fortifications. While modern growth has effectively obliterated any sense of the ancient city at major centers such as Athens and Thebes, in hundreds of valleys and hilltops from Asia Minor to Sicily it is still possible to gain an understanding of a Greek town in its natural context thanks to the preservation of its fortifications. This rich and varied source material has never received a comprehensive analysis in archeological, strategic, and historical terms. Dr. Winter has done this superbly. All the sites and remains discussed he has examined at least once himself, an indispensable and often grueling task, though one would not suppose so from Winter's own modest and straightforward account. He considers the locations of sites and the relationship of towns to their fortifications in what is a valuable contribution to the study of town planning (pt. 1). The fortifications themselves are examined from the materials and styles of masonry to the component elements of curtains, towers, gates, and so on (pt. 2). There are important modifications to the standard classification of walls by Robert Scranton (see especially pp. 95–100). Throughout, and particularly in part 3 ("The Critical Periods in the History of Greek Fortifications"), the historical significance of the study is presented clearly to the reader—the differences between the early Iron Age citadels of Old Greece and the east Greek settlements on the coast of Asia Minor, the consequences of growth in both areas, the new experience of colonists, especially in the west, the major impact of Lydian and Persian contacts and conquests in the sixth and fifth centuries B.C., the development of siege weapons in the later fifth century and the continuing struggle between offensive and defensive techniques through the Hellenistic period, the different character of the Hellenistic settlements themselves, and, not least in significance, the social implications of esthetically satisfying constructions. The main lines of historical interpretation seem to me unassailable, and I would only disagree with the importance Winter allows to trade as a factor before the colonial expansion.

The book itself is handsomely produced with generous photographs and plans that are kept close to the relevant parts of the text. The outer half of each nine- by ten-inch page is reserved for illustrations or left blank, a beautiful if extravagant expedient.

There is much of interest here for the general reader and traveler, and one may regret the absence of maps locating the often obscure sites and the failure to translate key passages of Greek. May one hope that the author will consider a "superior paperback" for a wider audience?

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## MEDIEVAL

JOSEPH R. STRAYER and DONALD E. QUELLER, editors. *Post Scripta: Essays on Medieval Law and the Emergence of the European State, in Honor of Gaines Post.* (Studia Gratiana, Volume 15.) Rome: Libreria Ateneo Salesiano. 1972. Pp. xvi, 788. L. 15,000.

This *Festschrift* is a worthy tribute to the distinguished scholar it honors. The volume is unusually well planned and is aptly named in more than one way. The various contributions really read like postscripts—additional reflections on themes that Gaines Post himself has diligently and creatively explored. Professor Post's earliest writing dealt with the comparative structure of universities. This led him to a consideration of the laws regulating medieval corporate groups in general and so to the cluster of topics that formed the subject of his life's work—Roman and canon law influences on the development of the medieval state, the inter-

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play between ecclesiastical and secular government, the medieval origins of the modern state. All the essays in the present volume are related in one way or another to these central themes, and this common focus gives an unusual coherence and unity to the collection. In the following notes we can give only an indication of the richness of its contents.

Several articles deal with the internal structure of Church law. The first two present typically learned contributions from two eminent scholars. Yves Congar prints many new texts illustrating the various meanings attached to the phrase *status ecclesiae*. Stephan Kuttner shows that a crucial passage on dispensation attributed to Urban II in the *Collectio Britannica* is an interpolation from Yvo of Chartres and discusses the whole related problem of *concordantia discordantium canonum*. In other studies in this group Richard A. Schmutz distinguishes between three types of papal representatives (legates, nuncios, and judges delegate), while Linda Fowler presents a substantial study on *recusatio iudicis*, the right of a litigant to reject a suspect judge in Roman and canon law.

Other contributions discuss the characteristics of the medieval state. John Perrin continues his research into "the terminology of power" and concludes that, for Azo, local rulers could possess an imperium not derived from the emperor. Michael Wilks discerns in Marsilius of Padua's *Defensor Pacis* a "Rousseauan" theory of general will and argues that, for Marsilius, this will was normally expressed by the *majores* of the community. Lester Little discusses the problems involved in the transition from the small-scale, intensely personal government of the early Middle Ages to the necessarily impersonal, bureaucratic structures of the later large-scale units. Joseph Strayer describes the mixture of political and economic motives that led to the levying of export taxes in England and France. Peter Riesenberg discusses fourteenth-century ideas on citizenship as a form of medieval egalitarianism.

In the area of Church-state relations Alfons Stickler explores the idiosyncratic views of the canonist Bernardus Compostellanus Antiquus who held that the emperor of Byzantium was the true heir of the ancient Roman emperors and that the western emperor was essentially an *advocatus* of the Church. Richard Kay argues that the "unnatural" crime of "Dante's Unnatural Lawyer" (Francesco d'Accorsio) was to use his legal skill perversely so as to exalt the authority of the pope and diminish that of the emperor. In similar fashion John Watt explains Dante's designation of Boniface VIII as "Lo principe d'i novi Farisei" by referring to a widely disseminated text of St. Jerome that described abuse of the power of the keys as pharisaical.

One substantial group of papers deals with the history of England and another with that of France. Edwin Hull points out that the phrase *quod omnes tangit* was used by Henry III of England as early as 1225—probably at the suggestion of Stephen Langton. Gavin Langmuir discusses the term *commune consilium regni* as used in Magna Carta and makes two significant points. *Consilium* meant counsel, not council; and a right to counsel did not necessarily imply a right of consent. J. C. Holt asks why Magna Carta—unlike charters of liberties in other lands—became a permanent part of statute law. He explains that the careful drafting of Magna Carta and its early reissues provided important clarifications of English law at a crucial stage in the development of the royal courts. W. O. Ault investigates cartularies and account rolls to find evidence concerning the activities of manorial courts in the early thirteenth century. Finally, Charles Wood reminds us, if we need reminding, that the complex constitutional documents of the reign of Edward II must be interpreted in the context of a power struggle between an incompetent king and an unimaginative baronage.

In the group of articles on French themes Thomas Bisson analyzes the very complex compound of old and new representative procedures in the great assemblies of Philip the Fair. He concludes that these assemblies should not be called "Estates-General" but that they "were indeed a new venture in national consultation." Elizabeth Brown explains how the principle, *cessante causa cessat effectus*, derived from Aristotle by way of civil and canon law, was used by both kings and subjects in fourteenth-century disputes over taxation. Jan Rogazinki's article on "Ordinary and Major Judges" deals with the influence of jurists trained in Roman law on the organization of

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royal and seignorial courts in the Midi. John Bell Henneman points out that in levying aids for the ransom of King John the French government could appeal convincingly both to old feudal custom and to the newly formulated Romano-canonical doctrine of "evident necessity." He concludes that the ransom aids were "a significant landmark in the merger of these legal traditions." In a somewhat parallel argument Fredric Cheyette explores the old Frankish and new canonical sources of the "royal safeguard" in thirteenth-century France. Howard Kaminsky also deals with the use of canon law by the French monarchy. He characterizes the writings of Simon de Cramaud on the Great Schism as "exercises in public relations." Simon used an extensive knowledge of early canonistic literature merely to justify policies that were in the interest of the French court.

Two papers are concerned with university education. André Gouron discusses the universities of the Midi as training grounds for French lawyers. Domenico Maffei presents an edition of Simon da Borsano's treatise on the proper qualifications of masters and scholars. Its first words indicate that some university problems are perennial ones. "Quero . . . an mulier possit ad doctoratum pervenire. Solvo quod non. . . . Est ergo officium virile feminis non competens."

A frequently recurring theme of the collection is the persistence of medieval ideas and institutions into the early modern period. Donald Sutherland discusses the classical and medieval origins of Grotius's doctrine on rights acquired by conquest. Kimon Giocarinis explores the Augustinian and Thomistic sources of Suarez's teaching on the origin of lordship. Ralph Giesey carries the story of *quod omnes tangit* down to the sixteenth century, investigating the use of the phrase and its relationship to the parallel Roman law dictum, *cuius interest, eius necessaria est auctoritas,* in the writings of François Hotman. Francis Oakley continues his researches into the medieval antecedents of modern "political theology" with a paper on "The 'Hidden' and 'Revealed' Wills of James I." Donald Queller edits an anonymous sixteenth-century treatise on the office of ambassador that is "replete with earthy admonitions generally based upon common sense and not found elsewhere." Karl Morrison presents a neglected letter by the Bolognese humanist Giovanni Antonio Flaminio describing the coronation of Charles V at Aachen. And J. Russel Major discusses the assembly of 1575 convoked at Paris by Henry III, describing it as "a direct descendant of a long line of medieval assemblies that were summoned more to persuade towns and provincial estates to accept a tax than to give actual consent for a levy."

There is one final way in which this collection forms a particularly fitting tribute to the scholar it honors. The topics discussed range from pure political theory to straightforward institutional history. A few—very few—of the contributors express impatience with one field or the other. But the overwhelming majority are concerned precisely with the interplay between theory and fact. The study of ideas and the study of institutions are both rewarding activities. They tend to be pursued by scholars of differing intellectual temperaments. But it is essential, if constitutional history is not to become a sterile field of study, that they be seen always as complementary, not conflicting disciplines. The whole corpus of Gaines Post's work exemplifies his understanding of this important principle.

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J. P. TRABUT-CUSSAC. *L'administration anglaise en Gascogne sous Henry III et Édouard I de 1254 à 1307.* (Mémoires et documents publiés par la Société de l'École des Chartes, 20.) Geneva: Librairie Droz. 1972. Pp. xli, 445.

This volume grew out of a thesis written at the École des Chartes in 1949. M. Trabut-Cussac later expanded his research during a stay in London as librarian at the French Institute of the United Kingdom. In 1959 he was asked to reorganize the library of the Casa Velázquez in Madrid and was able to equip himself with an exceptional knowledge of Spanish sources. In 1966 M. Trabut-Cussac was approaching the end of his long labors on the manuscript when he was savagely attacked in the garden of the Casa Velázquez. He died after thirty-two months of agony. His posthumous volume is now published with a brief *In Memoriam* by Professor Perroy that indicates the incomplete nature of the monograph, despite its 445 pages, including 46 pages of appendixes.

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There has wisely been no attempt in preparing the work for publication to remedy its lack of balance or to supply the generalizations and final observations M. Trabut-Cussac would have made. Nevertheless the book is an outstanding contribution to the political as well as the administrative history of Gascony. It traces in immense detail the variety and fragmentation of the area together with the conflicts engendered by local ambitions and fostered by the French rulers, who skillfully used their rights as defined by the treaties of Paris and Amiens. These are familiar topics. Not so familiar is the story of administrative reorganization and the fostering of order and cohesion by Henry III and Edward I, two rulers whose work in Gascony has never received the recognition it deserves. For the former, M. Trabut-Cussac has an admiration not often found in English historians. For the latter he has the very highest praise. Edward began his experiences in Gascony at the early age of fifteen. He is described as striving to bring order into chaos with a dedication surprising in one of his age, " . . . refaisant fil à fil la toile brutalement déchirée par Simon de Montfort, surmontant avec un froid courage les moments les plus pénibles, réussissant enfin à ramener une paix définitive, à recréer une administration normale, à maintenir son activité malgré des difficultés pécuniaires sans cesse renaissantes."

Such judgments provide the keynote of M. Trabut-Cussac's approach to his subject. He has penetrated deep beneath the surface to perceive the reason and skill that we might expect in a creative age as well as the violence and self-interest that are universal at all times and that circumstances greatly encouraged in the Gascony of the period. All M. Trabut-Cussac's judgments are based on a complete familiarity with local conditions and on a wide array of English, Gascon, and Spanish sources. There is no bibliography, but there is copious reference both to unpublished records and to printed works, including the writings of English-speaking scholars like G. P. Cuttino and Pierre Chaplais. Added to this there is an excellent index.

Thus, despite its inevitable limitations, the volume will be indispensable for all future research in this area. It has many points of resemblance with that on the later period (1399–1453) by G. A. Vale. Together these two monographs may be said to constitute a landmark in the history of administration and politics in the Gascony of the later Middle Ages. We may all be grateful for what M. Trabut-Cussac has given us, admiring his industry and scholarship, lamenting the loss to historical writing through his untimely death, and resenting the senseless brutality perpetrated in the garden of the Casa Velázquez.

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MAURICE BERESFORD and JOHN G. HURST, editors. *Deserted Medieval Villages: Studies*. New York: St. Martin's Press. 1972. Pp. xviii, 340. $23.95.

This handsome volume may be welcomed as the first major publication of the Deserted Medieval Village Research Group of which the two principal authors were founder members in 1952. The group has concerned itself with the identification and listing of deserted village sites in the British Isles and has taken a major part in the organization of both rescue and research excavations on such sites. This book, which has been long in the press, takes account of the work done up to 1968. Together its four parts covering England, Scotland, Wales, and Ireland comprise a useful synthesis of results already achieved, supported by valuable gazetteers and bibliographies. In a book that is so obviously original and important it is a pity that the level of attainment in both editing and authorship is so uneven.

The weightiest part of the argument, as is only to be expected, concerns the English sites, the discussion of which takes up rather more than two-thirds of the book. This first part opens with a rambling and tendentious account, the work of Professor Beresford, of the state of play in the current historical debate on the reasons for the desertions and their timing. Professor Beresford makes the most of his opportunity to come to grips with the critics of his *The Lost Villages of England* (1954), yet he does so in such an allusive and anecdotal style as to forfeit the sympathy of his academic reader. When Professor Beresford, in a later passage, speaks without references of "Finbergian continuity à la Withington," there is nothing unusual in his manner. But how, one

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wonders, would he himself react to such mirthless jargon were he to discover it in somebody else?

Very much more interest attaches to the remarks of his coeditor, Mr. Hurst, who has many original things to say. Nobody, it is certain, knows the archeology of the English deserted village better than Mr. Hurst. He has initiated and visited repeatedly most of the major excavations on deserted village sites conducted over the last two decades, and during the same period has himself directed the important campaign of excavations, still continuing, at Wharram Percy, Yorkshire. Very properly he is at pains to emphasize to fellow archeologists the desirability of open-area excavation on such sites, but he has important things to say also to the historian, whether it be on the subject of improving building standards or on those continual changes, which have taken us all by surprise, in the alignment of peasant houses and even in the plan of the village itself. Wisely he stresses the unexploited potential of such sites for population studies, saying something, too, of the value of closely dated animal bone assemblages in the study of village ecology throughout the period he treats. Mr. Hurst does not write as a historian, and sometimes, as in his discussion of the use of hand querns and the manorial mill, a faulty sense of period threatens to lead him badly astray. But Professor Beresford surely does his colleague an injustice when he comes near to describing his work as "a disarray of meagre skirmishes in a campaign where the enemy has yet to be properly engaged." Mr. Hurst, at least, has got from his material as much as it can reasonably be expected to yield.

The same could scarcely be said for the much slighter contributions of Messrs. Fairhurst and Dunbar on Scotland, of Dr. Bulter on Wales, and of Dr. Glasscock on Ireland. None have thought it necessary to supply a map of the regions they discuss, and each makes the same point that the deserted village on the English pattern is not truly characteristic of his area, which is close to a confession that the chapter need not have been written at all. Indeed it is precisely in this implied imprecision of direction and design that the principal weakness of the volume seems to lie. In part a manifesto, in part a bulletin of work in progress, this book only occasionally ascends to a discussion of its announced theme. But what, after all, is that theme, when none of the three words of the title can be said accurately to describe it? Professor Beresford, it is obvious, is the only author primarily interested in desertion; none of the contributors confine their remarks to the medieval period; and no one, outside England, can find himself a village.

COLIN PLATT
*University of Southampton*

W. O. AULT. *Open-Field Farming in Medieval England: A Study of Village By-Laws.* (Historical Problems: Studies and Documents, 16.) New York: Barnes and Noble. 1972. Pp. 183. $9.50.

Professor Ault published in the *Transactions of the American Philosophical Society* (n.s. 55 [1965]: 1–102) a paper on "Open-field husbandry and the village community: a study of agrarian by-laws in medieval England." In this he printed over two hundred examples of local rules for the ordering of village agriculture that were culled from the court rolls of some forty manors and ranged in date from 1270 to 1608; they were preceded by an analysis of the contents of these bylaws and of their implications for the development of the village community in late medieval England. The present book is in effect a new edition of that paper. Some changes have been made. The Latin bylaws are now given in translation and those from 1546 have been replaced by translations of eleven specimen court rolls dating from 1283 to 1379. The introduction has been abridged and rearranged and a little extra material incorporated; among this is a particularly interesting account of Kingsthorp (Northamptonshire) and its curiously well recorded communal organization (pp. 72–75). But the gist of Professor Ault's work is unchanged. Among much else of importance it includes evidence for joint plowing among peasants, a demonstration that the customs of the manor concerned free tenants as well as villeins, and an account of the emergence of the parish church as the administrative center of the village community.

Unfortunately nothing has been done to correct the many minor errors that so badly marred the paper of 1965. Many references are

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incorrect (for example, British Museum Add. MS. 40010 appears as Add. Ch. 40010 on page 41 and as Add. Ch. 60010 on page 59, while there is much confusion between local record societies and record offices, as on page 54). Some place names are wrongly identified, a particularly serious fault where so much may hinge on geography; thus Whitchurch (Warwickshire) is placed in Hampshire on page 24, Malden (Surrey) in Essex on page 34. The translations are occasionally faulty (for example, "of the granaries" for a form of *grangiarum* in number 4, "sheep" for *agnellas* in number 56), and as the accuracy of the extracts published in 1965 is not always beyond question, the texts as we now have them may depart considerably from the meaning of the original documents. The book, like others in the series, is meant to show students the relationship between original documents and scholarly monograph. They will learn much of the general outline of agrarian and communal organization in late medieval England. But they will learn much less of the care and accuracy that should be fundamental to all historical research.

P. D. A. HARVEY
*University of Southampton*

C. R. CHENEY. *Notaries Public in England in the Thirteenth and Fourteenth Centuries.* New York: Oxford University Press. 1972. Pp. viii, 205, 6 plates. $17.00.

In this volume Mr. Cheney provides a comprehensive survey of the development, duties, and influence of the office of notary public in thirteenth- and fourteenth-century England with some comparison between it and the office on the Continent. He stresses the fact that the notary public was a public person performing some public functions in the field of private law on authority given him by the pope or the emperor. English common law did not recognize the notary public as it required a sealed document; the Church, on the other hand, in part because of the difficulty of authenticating a seal unless it was known, preferred a notarized one. The crown did employ notaries public, particularly in chancery, probably because most chancellors were churchmen, some of whom were themselves notaries. Notaries holding office under the crown augmented their stipends by functioning privately.

Notaries public first appeared in England in connection with Church business in the middle of the thirteenth century when foreign influence was strong. Mr. Cheney has identified the first Englishmen to serve in this capacity and has shown how the use of such officials gradually expanded. In 1279 Archbishop Peckham received papal faculty to appoint three notaries public, who became members of his permanent staff. Other prelates followed his example until in the fourteenth century an English bishop usually employed at least one notary public who was compensated with a benefice, if in orders, or with a salary, if not.

Candidates for the position, whether seeking appointiment directly from the pope or from a bishop with a papal faculty to appoint, underwent an examination about which little is known. In addition to scribal competence and good character, a notary public was expected to have some legal knowledge, which he could obtain at a university or by apprenticeship in an episcopal chancery.

Mr. Cheney discusses in detail the diplomatic of notarial documents with attention to the influence of Italian models on English practices. The methods of dating used in English and Italian documents can cause confusion if scholars are unaware of the differences. Diplomatic and other matters are illustrated with sample forms of notarial instruments; there are facsimiles showing the styles and practices of better-known English notaries public. Mr. Cheney suggests that during the fourteenth century clerks trained as notaries public may have had considerable influence on legal terminology and on the style of legal documents of both Church and state and that private mercantile transactions may have been similarly influenced. Included in this book is a bibliography of printed books and articles.

ELISABETH G. KIMBALL
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JOHN HATCHER. *Rural Economy and Society in the Duchy of Cornwall, 1300–1500.* New York: Cambridge University Press. 1970. Pp. xiv, 322. $16.00.

Late medieval Cornwall has attracted little re-

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search interest save for its mining. Dr. Hatcher has now made ingenious use of leasehold information in the accounts of seventeen duchy manors to shift interest to the effects of mining, and of other Cornish industry, on the local demand for land. By the 1330s it was duchy policy to put the majority of tenants on competitive rents at rates that were renegotiable every seven years. Since all of the land not reserved for deer parks was for rent, the accounts offer an unusually systematic record of fluctations in demand. On a number of the manors there was clearly a strong relationship between their revenues from land and the local output of tin. Here Hatcher is on firm ground, for he has been able to fill gaps and correct errors in previously printed figures of Cornish tin production. The other industries that he believes created a strong demand for foodstuffs were clothmaking and the processing of fish. For these there are no firm statistics. However, it is reasonable enough to assume that along with the victualing of ships, and the revival of mining across the Devon border in the later fifteenth century, they go far to explain the phenomenon of high and rising rental revenue on the more fertile manors of the southeast, after Cornish mining slumped in the late 1420s.

The book, which is a doctoral dissertation, deploys its quantifiable evidence in well-designed form, and has spirit. But it wavers oddly between assuming a one-to-one correlation between population and land values and erecting simplistic use of this assumption, without regard to changes in land use, into a straw man for attack. The brief preplague tin rush obviously helped to raise some Cornish land values through demand both for housing and for the farm plots that gave miners, and all rural craftspeople, more security. Still, the growing rationalization of administrative policies would appear to have been the main factor in raising rental revenues on these manors through the early fourteenth century. Again, there is too little comparison with other parts of England. Neither the quick filling-up of tenancies of the better land after the first plague nor the speculative long-term leasing by men with incomes from administrative service and trade in the late fifteenth century were peculiar to Cornwall. In sum, Hatcher has added southeast Cornwall to the pockets of life in the generally feeble economy of the two middle quarters of the fifteenth century and has made a welcome move toward studying medieval agriculture not in isolation but in its relation with other occupations.

SYLVIA L. THRUPP
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V. J. SCATTERGOOD. *Politics and Poetry in the Fifteenth Century, 1399–1485.* (Blandford History Series: History and Literature.) New York: Barnes and Noble. 1971. Pp. 415. Cloth $12.75, paper $5.00.

The considerable corpus of political verse that still survives from the fifteenth century probably represents only a fragment of that originally written. Much of it was ephemeral in interest, circulated orally and was never committed to paper. Yet by the later part of the century possibly as much as forty per cent of the population could read and the increasing propaganda influence of the written word had become obvious even before the introduction of printing—so much so that a proclamation was issued to forbid the posting of bills for men to read on church doors and other public places. Prominent people commissioned such verses, and writers who attacked the government were, from time to time, executed for their unwelcome efforts.

Dr. Scattergood has written the most comprehensive book so far to appear on this important subject—with chapters illustrating nationalism and foreign affairs, domestic politics, the Church, anticlericalism and Lollardy, the social structure and social attitudes. All kinds of historians will find something to interest them in this volume and will neglect its numerous quotations from the sources at their peril.

Nevertheless the book has a number of sad defects. There is about it a certain slackness of thought and vagueness of definition. In his long chapter on nationalism Dr. Scattergood never defines the term precisely enough. I see very little of its positive modern meaning in the numerous extracts he quotes. In the main they illustrate little more than a distaste, at times hatred, for things foreign; surely mere xenophobia rather than nationalism, as this word is generally understood.

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The main defect of the book, however, is unequal erudition. Dr. Scattergood's knowledge of fifteenth-century English history is notably inferior to his learning in its verse. A glance at the bibliography confirms this impression, showing an absence of such names as C. A. J. Armstrong, J. M. W. Bean, F. R. H. Du-Boulay, A. R. Bridbury, R. Virgoe, and B. P. Wolffe whose works, over the last fifteen years, have done so much to extend our knowledge of the politics, government, and economic life of the period. Likewise absent are the fundamental works of M. Bowker and P. Heath on the clergy and W. H. Dunham, Jr. and G. L. Holmes on the aristocracy and bastard feudalism. Consequently the political commentary in which the poems are set is often both old-fashioned and jejune, and many comments on social and religious matters strike a somewhat out-of-date air: defects the more to be deplored as this book will certainly be widely used.

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EDWARD GRANT, edited with an introduction, English translation, and commentary by. *Nicole Oresme and the Kinematics of Circular Motion:* Tractatus de commensurabilitate vel incommensurabilitate motuum celi. (The University of Wisconsin Publications in Medieval Science.) Madison: University of Wisconsin Press. 1971. Pp. xx, 415. $20.00.

Of the late medieval scholastics whose treatises and commentaries have survived for us, few have been as original and provocative in their treatment of physical and mathematical problems as the French schoolman Nicole Oresme, who died as bishop of Lisieux in 1382. We are thus fortunate to have another edition of a work by this interesting medieval thinker presented with text, introduction, commentary, critical apparatus, as well as readable English translation, to add to the three Oresme treatises previously published by the University of Wisconsin Press in their Publications in Medieval Science series.

This very fine edition of Oresme's *De commensurabilitate vel incommensurabilitate motuum celi* is particularly valuable in that, among his fellow scholastics, Oresme appears to have been unique in his penchant for kinematic problems, whether applied to the celestial realm or not, that appear throughout many of his treatises as illustrations of a particular argument. This predilection is most explicitly revealed in his two treatises, the *Ad pauca respecientes,* also edited by Professor Grant together with Oresme's *De proportionibus proportionum* in the Publications in Medieval Science series, and the present work.

The *De commensurabilitate,* written before 1377, the date when Oresme completed his French commentary on Aristotle's *De caelo* and his *Le Livre du ciel et du monde,* a work in which the *De commensurabilitate* is cited by name, is divided into three sections. The first two contain a number of propositions concerning the motion of celestial bodies or mobiles, first, on the assumption that their speeds are commensurable and that, for example, it would be possible to determine the conjunctions of such bodies; and second, on the opposite assumption that such motions are not commensurable and thus that astronomical configurations are not repetitive. The final part of the treatise drops the mathematical format of the prior sections and presents an argument on the merits of both points of view between personifications of arithmetic (commensurability) and geometry (incommensurability) before Apollo, as dreamed by Oresme.

Motivated by his confirmed objections to astrology, especially the court astrologers surrounding the French king Charles V, Oresme did not himself believe that celestial motions were commensurable. In *Nicole Oresme and the Astrologers; A Study of His "Le Livre de Divinacions"* (1952), edited and translated by G. W. Coopland, Oresme is aligned with a Christian tradition that found odious the ancient notion of such cyclical concepts as the Great Year, a period of time of thirty-six thousand years after which all celestial bodies would return to the same position. The Christian abhorrence of this cyclical notion reached its climax with the inclusion of a statement on the Great Year among the theses condemned in 1377 by the bishop of Paris.

But in his mathematical approach to celestial commensurability or the lack of it, as Professor Grant points out, Oresme had virtually no predecessors apart from the second-century author Theodosius of Bithynia (or Theodosius of Tripoli), whose *De diebus et noctibus* treats

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this theme, as does a section of the *Quadripartitum* of Oresme's older contemporary, Johannes de Muris, although in the case of de Muris, exact priority cannot be established. Thus Oresme's own claim to originality for his treatment of celestial kinematics seems justified.

Nor had Oresme any subsequent imitators in this area; although several scholastics obviously were influenced by his work, such as Henry of Hesse who quotes Oresme by name, or Pierre d'Ailly who plagarized Oresme's treatise, or John de Fundis who wrote an attack on the *De pauca respecientes*, no one apparently composed a work on the same topic. Thus, on the evidence available, Oresme's treatises on the commensurability or incommensurability of celestial motion represent the sole significant contribution on this theme during the medieval period. In view of this, the publication of Professor Grant's excellent edition, which makes this treatise by Oresme available to a wide audience of scholars, is especially welcome.

CLAUDIA KREN
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KLAUS ARNOLD. *Johannes Trithemius (1462–1516).* (Quellen und Forschungen zur Geschichte des Bistums und Hochstifts Würzburg, Number 23.) Würzburg: Kommissionsverlag Ferdinand Schöningh. 1971. Pp. xi, 319. DM 35.

For centuries Johannes Tritheim, 1462–1516, has had an ambivalent reputation as monastic administrator, tireless writer on religious and historical subjects, and as a fabricator of spurious medieval sources and dabbler in magic. Contradictory testimony of contemporaries and scattered documents made it difficult to see the whole man. In the present book, originally a Würzburg dissertation, the pieces of a life are brought together for the first time. A precocious youngster but too impatient to stick to a formal academic career, Tritheim entered the Benedictine monastery of Sponheim in 1482. Within a year he was its abbot. He revealed himself as a rigorous reformer not only of monastic discipline but also of the monastery's declining economic position. He built a splendid library and wrote numerous books of instruction for his monks. His order raised him to eminence; he regularly acted as chapter orator, definitor, and visitor.

After a quarter century at Sponheim he moved to St. James in Würzburg where he repeated many of his earlier accomplishments and continued his copious writing, mostly of anthologies such as his catalog of ecclesiastical writers (*de scriptoribus ecclesiasticis*), his *catalogus illustrium virorum Germaniae* produced at the instigation of Wimpheling, and, his best-remembered works, chronicles of the monasteries of Sponheim and Hirsau and a compendium of Frankish history. As historian Tritheim was not only uncritical but also ready to invent sources where his claims could not otherwise be substantiated. His bogus chronicle of "Hunibald" let him prove that the Merovingian kings stemmed from Hector of Troy and that the Franks had entered Europe in the fifth century B.C., while his fictitious "Meginfrid of Fulda" furnished data on the earliest history of Hirsau. The latter forgery was not exposed until the nineteenth century; "Hunibald," however, did not fool the more acute scholars of his own time. Tritheim's objective as chronicler was to show how early monasticism had created not only a high literary culture but also pure religiosity. "Meginfrid" provided a paradigm of monastic life, "Hunibald" of distinguished antiquity. Both were frauds but, in a way, honorable frauds. They did not cost him his many learned friends, nor his reputation as a polymath.

Arnold's bio-bibliographical monograph with its excellent apparatus seeks only to establish the facts. But a kind of portrait emerges of an able ecclesiastical functionary who, though not especially gifted in intellectual matters, was moved by position and disposition to write busily on nearly all the subjects favored in late medieval thought and who through these writings came to play a modest role in forming the literary culture of his time. Arnold is surely right in avoiding speculation. No search for motives could reveal more about the spurious Hunibald than we know. Arnold registers surprise and some dismay at Tritheim's interest in the occult, but as a result of recent scholarship on magic we are in a much better position to see its place in the world view and life work of a man like Tritheim whose works on cryptography (which, largely misunderstood, were held to be conjuring books) and on demons and witches are perfectly compatible with his schol-

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arly bent and religious inclination. A dedicated, industrious man, simple despite his learning, eternally curious, and possessing no trace of skepticism, Tritheim was very much a child of his time, one of the many minor figures whose ways allow us to grasp the character of their era.

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## MODERN EUROPE

HENRY RAYNOR. *A Social History of Music: From the Middle Ages to Beethoven.* New York: Schocken Books. 1972. Pp. viii, 373. $12.00.

Raynor attempts "to fill some part of the gap between the normal and necessary history of music which deals with the development of musical styles and the general history of the world in which composers carried out their function." The project is important, the result disappointing.

The materials dealt with—such as the origins of musical forms, the development of musical institutions in the Church, towns, and courts, the lives of leading composers (Bach, Handel)—are fascinating, of course, but too much is given undigested and without indication of relevance to the main theme. The general history presented, as well as the attempts to connect it with musical developments, will make historians uneasy. For example: "The medieval city . . . was a place in which a collection of more or less equal and equally responsible merchants and tradesmen lived for their convenience. It was not a place rising from the need of employers to create housing for the mass of poorer people who did the less-skilled work which they had to offer. The result was a beauty which eighteenth- and nineteenth-century cities failed to recapture. The centers of the wool trade in England can stand as examples—Cotswold towns like Chipping Camden . . . Lavenham—to show the sense of style and dignity which went into the creation of cities, in which all the amenities were to be shared by more or less equal and equally wealthy citizens." And this explanation of the rise of new styles about 1600: "The decisive factor in the victory of the Baroque was its consonance with the outlook of the age."

Made uneasy by passages such as these and accounts of the Babylonian Captivity of the Church and the Great Western Schism, for example, historians will wonder about the reliability of the musical materials. They will observe that footnotes are sparse and fail to give volume and page number; they may not realize that the bibliography (list of books and an article) omits many books bearing vitally on the subject, such as Dean on Handel, Kerman on the Elizabethan madrigal, Einstein on the Italian madrigal, and Heartz on Pierre Attaingnant, as well as standard European encyclopedias such as *Die Musik in Geschichte und Gegenwart.* They may note that almost none of the recent musicological contributions published in scholarly journals such as the *Journal of the American Musicological Society* and *Music and Letters* are listed or cited. In short, they cannot assume that this book takes into account a major part of the meticulous musicological scholarship of the last quarter century.

An uncritical general reader could get much pleasure from it. A historian or musicologist could not recommend it.

WALTER L. WOODFILL
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MICHALINA VAUGHAN and MARGARET SCOTFORD ARCHER. *Social Conflict and Educational Change in England and France, 1789–1848.* New York: Cambridge University Press. 1971. Pp. 261. $11.50.

This book is relevant, as it is meant to be, to the worldwide concern over the relation of educational policy to social conflict and change. The two English sociologists (at the London School of Economics and the University of Reading) disavow any intent of writing history or even historical sociology. They do, however, find the present thrust of the "sociology of education" too restricted to contemporary complex economies, believing that "such limitations in the historical perspective of studies preclude the development of a theory of institutional change." Though building on Max Weber's theoretical and historical studies of social conflict, they wish to progress beyond the reconstruction of the past to the construction of a "theory of change" that will have "predictive power." With this ultimate aim they make a

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rigorous comparative analysis of educational change in England and France in the matrix of the institutional changes of the Industrial and French Revolutions. They do recognize the need for many more case studies and are planning to continue this study down to the present. Contrary to the Marxist and "structural functionalist" theories, they conclude that by the mid-nineteenth century Britain's greater industrialization did not lead to the degree of secularization and state control achieved in a less industrialized France and that religious or political ideologies and interests could outweigh economic factors in educational change.

Intellectual historians will probably rejoice in the large proportion of space devoted to the analysis of the effective power of educational ideas as interrelated with economic and religious ideas, groups, and movements. They may be startled to find Rousseau practically ignored as an educational theorist while his contemporary La Chalotais is treated as a giant.

Historians, however, will be likely to quarrel with the authors, as they have with Weber, over their use of "ideal types"—or perhaps more—since the disciples prove to be more dogmatic and rigid than Weber in their use of "class" and other "ideal types" of conflicting social groups or ideologies, indeed, far more simplistic than their first two chapters would lead one to expect. Their analysis of the "bourgeois" collective mentality in France commingles "Parliamentarians" (nobility of the robe), philosophes, Gallicans, Jansenists, the Abbé Siéyès portrayed as a defender of the "People," the aristocratic Talleyrand, the undefined "revolutionary bourgeoisie," the imperial bureaucracy, and the July Monarchy. In England the "middle class," despite the authors' disclaimer in theory of the identity of religion and class, is in practice equated with Nonconformists and Utilitarians. "Ideal types" may be defended as a heuristic device, but not as substitutes for empirical knowledge nor rigid categories for empirical data. Yet historians may find here challenging hypotheses to apply to their own craft.

EVALYN A. CLARK
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GUILLAUME DE BERTIER DE SAUVIGNY. *Metternich et la France après le Congrès de Vienne.* Volume 3, *Au temps de Charles X—1824/1830.* Paris: Presses Continentales. 1971. Pp. 920–1426.

With this volume Professor Bertier completes what is clearly a definitive study of Franco-Austrian relations during the Restoration. Once again he displays a sovereign mastery of the sources, especially archival ones, and a pleasing objectivity and good humor. The issues over which France and Austria were at odds are depicted clearly and fairly; the portrait of Metternich is penetrating and deftly ironic. All students of diplomatic history will welcome this study and use it with profit.

There are, however, certain reservations. Along with providing fresh, helpful information, Bertier discusses in detail issues he himself regards as insignificant, and he studs the book with long extracts of unpublished dispatches. This is by design; where he wishes to, Bertier writes with admirable compression. His aim is to enable the reader to understand the mental world of the historical actors through their own words, discussing everything, however trivial, that they took seriously. The trouble is that the actors themselves often knew that they were dueling for show, over meaningless issues. Even an urbane guide like Bertier cannot entirely relieve the tedium of so thorough a tour through this museum.

I also disagree somewhat with Bertier's interpretation on the most important issue in the book, the Eastern question. Pertier primarily blames Metternich for the failure to achieve Austro-French cooperation, arguing that Austria and France together could easily have restrained Russia while settling the Greek issue, thereby avoiding all the complications and dangers of isolated Russian action in 1827–29. The scenario appears to me implausible. Of course Metternich's policy was passive and negative; some of his predictions (often made for tactical reasons) proved incorrect. But the main point proved by 1825–29 is Metternich's prescience and the reckless folly of the West. His fundamental argument—that any European partnership with Russia to coerce Turkey in behalf of the Greeks, far from restraining Russia, would only promote war and, consequently, bring certain Russian victory and ascendancy over Turkey and the possible disintegration of Turkey—was fully vindicated by

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events. So was his insistence that such a course would promote revolution and nationalism in Europe and endanger the thrones of France and Russia. Bertier himself provides much evidence of Austria's foresight and the West's levity—the fact that Britain and France were shocked at the Peace of Adrianople, while Metternich had expected much worse; the way in which French statesmen first contemplated with distaste, and then embraced, a partition of the Ottoman Empire by 1829; the hand-wringing dismay with which Frenchmen recognized that their policy had served to deliver the fate of the Ottoman Empire into the hands of Russia alone.

This suggests one more comment. Bertier rightly concludes that Franco-Austrian rivalry derived fundamentally from ideological incompatibility. He also sees correctly that Metternich, for all his skill and realism, was mired in the hopelessly anachronistic conservatism of Francis I. But he regards the efforts of Frenchmen like Richelieu and La Ferronnays to give France an independent, generous, and glorious policy as not merely normal but legitimate and praiseworthy. He does not seem to have seriously considered the dangerous and anachronistic aspects of this endeavor. Still, these are minor criticisms of a very impressive accomplishment.

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RAYMOND J. SONTAG. *A Broken World, 1919–1939*. (The Rise of Modern Europe: A Survey of European History in Its Political, Economic, and Cultural Aspects from the End of the Middle Ages to the Present.) New York: Harper and Row. 1971. Pp. xviii, 415. $8.95.

The title of this latest addition to the venerable, and now almost complete, Langer series is taken from Gabriel Marcel's play of 1933, *Le monde cassé*. It conveys the feeling, widespread in the interwar period, that "the world of European thought, feeling, and action was broken, but not broken beyond repair." Despite the melancholy theme, the narrative is crisp, the style elegant; humor is not lacking, nor is hope.

The author, unfortunately now dead, was a well-known diplomatic historian, and, accordingly, international affairs occupy a large place in his pages. Diplomatic history, moreover, offers an obvious means of drawing together the multifarious currents of events in a study which, by the very premise of the series, deals with a supranational subject. No one can deny that the search for peace, stability, and "normalcy" was a salient feature of the interwar years, and Professor Sontag makes his way through the labyrinth that led from the Peace of Paris to the Nazi-Soviet Pact. A bit too much space, perhaps—one-third of the contents—is devoted to international relations, and many additional sections are given over to the domestic repercussions of international affairs. As a result domestic political events and social and economic developments suffer inevitably. Nontotalitarian political parties are not analyzed at sufficent length, nor are they compared cross-nationally. International finance and trade, the problems of production and industry, social policy and welfare legislation, the Great Depression itself, find a congested home in the chapter "Science and Technology." Women's invasion of the labor market, no mean achievement of World War I and the interwar years, is not mentioned. On the other hand, the sections on intellectual and artistic life are detailed and compelling. Here and elsewhere, the author allows representative figures of the age to speak at some length for themselves; Keynes, Freud, Einstein, Yeats, Spengler, Hemingway, and members of the Bauhaus school define the dilemma of their generation with very good effect.

The author's basic outlook in this book is one that has been common in the quarter-century just past. Briefly, this view holds that World War I unleashed an era of violence and upheaval. Out of this chaos emerged totalitarian states; eventually the democracies had to come to grips with fascism. (Later, they would be confronted with another totalitarian threat in Soviet Russia but this, of course, is outside the scope of Sontag's work.)

The many recent revisionist writings notwithstanding, this reader finds no quarrel with Professor Sontag's basic premise. Still, the "totalitarian-democratic" dichotomy involves a heavy positive moral valuation that leaves the "totalitarian" end of the scale empty. Such a Manichean vision of history does not accommodate a whole range of dichotomies that would

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more closely reflect the tremendous upheavals of the twentieth century.

Starting with an excellent description of the Peace of Paris, the author proceeds to a substantial and convincing analysis of the Russian and German revolutions. Since he dislikes fanatics he tends to see the German revolution "from inside," and he tends to see it sympathetically; he sees the Russian "from outside" and rather unsympathetically. Here as well as later (see his discussion of Stanley Baldwin and Léon Blum) he favors moderates. No doubt this is a useful antidote to the views of many people that the whole "pause between wars" was a monumental swindle, and moderate politicians were at best bunglers. Yet one must note with certain uneasiness that, because of the devil in others, Stanley Baldwin surfaces as one of the pale heroes of the book.

Some sections on political history are excellent. The account of the Spanish Civil War is a model of sanity and balance in the small space it occupies; the pages on Neville Chamberlain are really fine—one of the rare accounts I have seen that makes Chamberlain's policy intelligible and rescues him from the stock role of England's evil spirit; the section "Some Moot Questions on Europe in 1938–1939" (ch. 12, pt. 2) is a first-class discussion of Hitler's objectives, the nature of Soviet foreign policy, and why it all needed to end in a second great war. The treatment of the Czechoslovak crisis is magnificent. The close of the book, on negotiations leading to the Nazi-Soviet Pact, is again one with which I agree in my heart, but again where I would have welcomed some new evidence, or a fresh approach, in order to dispel my own lingering doubts. Was the USSR truly determined upon a German alliance beginning in March 1939? Were the Western Powers not overly dilatory in their dealings with the Soviets from March to August 1939? Might Stalin not have wished to buy time (and space), just as the British had done at Munich? Did Soviet Russia not have a plausible claim to lands inhabited by Russians and other East Slavs and/or historically part of Russian polity? Was the Soviet desire to secure a belt of client states on the side facing Europe so much more heinous a motive than Britain's historic wish to see a friendly Holland and Belgium? Well, the blame must still be thrown to the Soviets, if for no other reason than their refusal to provide evidence from their own archives.

If the treatment of great-power policy is generally exhaustive and excellent, the small countries draw less space than would perhaps be their due. And there are mistakes here. No, the Allies did not decide early in 1919 that "the whole Transylvanian plain was to go to Rumania" (p. 61). They decided to give the Romanians far more than just Transylvania, with its towering peaks, deep valleys, gentle hills, and its "plain." The resignation on March 21, 1919, of Hungary's democratic government did not leave "an unstable coalition of socialists and communists in control," within which the socialists were soon to be pushed aside by Béla Kun who "set up a Communist dictatorship." The coalition of socialists and communists—with the socialists in an increasingly strong position—lasted as long as the Republic of Councils. The war of the Hungarian Soviets went on for quite a while, and it consisted of more than just the Romanians advancing "with little difficulty." Oh, well.

The book is furnished with fifty-five black-and-white illustrations; the selection of plates of interwar posters, paintings, sculpture, and architecture and design is especially fortunate and reinforces the impression that the talents of this diplomatic historian also extend to intellectual and art history. The bibliography is excellent and up-to-date. Here, paradoxically, the only omissions seem to be in the area of international affairs. Such works as E. H. Carr's book on interwar international relations and Louis Fischer's on Soviet foreign policy are absent; both are perhaps a trifle dated and deficient in scholarly apparatus, but they are still useful and readable works for the student and general reader, for whom the Langer series and this fine study are primarily designed.

ISTVAN DEAK
*Columbia University*

ALLEN F. CHEW. *The White Death: The Epic of the Soviet-Finnish Winter War.* [East Lansing:] Michigan State University Press. 1971. Pp. x, 313. $12.50.

THEDE PALM. *The Finnish-Soviet Armistice Negotiations of 1944.* (Acta Academiæ Regiæ Scientiarum Upsaliensis, Number 14.) Stock-

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holm: Almqvist & Wicksell. 1971. Pp. 160. 40 Sw. kr.

These two books deal with Finnish-Soviet relations during the war years, although the focus of one is on the Winter War of 1939–40, the other on the Continuation War of 1941–44. The Chew volume will appeal primarily to military historians who are not daunted by the details of battle, and the Palm volume will interest diplomatic historians who are willing to overlook a poor English translation of materials written in Swedish and Finnish.

The title of Chew's book is happily misleading. The reader will find a well-researched account of the campaigns of the Winter War, not a journalistic and sensational tribute to the prowess of Finnish soldiers. The author recaptures some of the spirit of 1939–40 and ably presents a factual description of the most significant military events of the war. The hero of this account is Colonel Alpo K. Marttinen, U.S. Army (Ret.), "the daringly aggressive chief of staff of the 9th Division" who played a major role in the Finnish victory over the 163rd and 44th divisions. The inevitable Soviet tanks had failed at Suomussalmi.

Chew is much weaker, however, on political questions. By painting a distorted portrait of Foreign Minister Väinö Tanner, who is said to have engaged in a three-month search for peace, Chew sidesteps the failure of Finnish foreign policy. Tanner's responsibility in this debacle was substantial. Furthermore, it is surprising that Chew gives short shrift to political factors as a possible explanation for Moscow's decision in mid-February to halt the Red Army's advance when "total victory was apparently within its grasp." The epilogue, which deals more specifically with politics, is especially weak. Many readers will not agree that Finland's existence as an independent state is precarious, that Finland is paying political ransom, and that the Finnish government has generally deferred to Moscow's slightest whim. It would be interesting, moreover, to learn the identity of the many thoughtful Finns who fear that their nation may eventually become "the 16th Soviet Socialist Republic."

The Palm volume makes available in English the minutes of the two meetings between Finnish and Soviet representatives in March 1944 and the minutes of the September 1944 Armistice Conference between Finland, the Soviet Union, and Great Britain. These minutes were written in Russian by the Finnish delegation's secretary Georg Enckell, checked for accuracy by the Finnish plenipotentiaries, and subsequently translated into Finnish. A copy of the documents was carried beyond Finland's borders by Enckell, who became a Swedish citizen in 1945, and it was kept in the private custody of Thede Palm for a quarter of a century. The minutes are less spectacular than the transcript of the Berlin meeting between Molotov and Ribbentrop, yet they nonetheless provide insights into Finnish and Soviet behavior at a critical juncture in history. Background material is provided in a fifty-one page introduction.

The works of Palm and Chew overlap only slightly, but the two authors agree on a number of points. Both men are struck by the bravery of Finnish soldiers in the Winter War, which is seen as an effort by the Soviet Union at the total incorporation of Finland under Russian rule, and they seem to accept the argument that the Soviet air raids of June 25, 1941, were an opportune pretext used by Finland for war against the Soviet Union as a cobelligerent with Germany. There is an apparent dislike for J. K. Paasikivi, the chief negotiator for Finland in the spring of 1944, although he is regarded by other observers as Finland's greatest statesman. It may indeed be true that Paasikivi's command of the Russian language was weak, but the statement that he sought "peace at any cost" certainly indicates naiveté on Palm's part concerning Paasikivi's views. The omission from Chew's bibliography of Paasikivi's memoirs, which are openly critical of Väinö Tanner, is equally suggestive.

The amount of labor that went into Chew's book is impressive. Taken as a whole, however, it is a tedious volume of interest to only a small audience. The contribution made by Palm will have a more lasting impact, although the documents suffer in presentation and interpretation.

JOHN H. HODGSON
*Syracuse University*

W. A. PANTIN. *Oxford Life in Oxford Archives.* New York: Oxford University Press. 1972. Pp. x, 123. $7.00.

The decision to form a book on the University Archives of Oxford from these delicate reports

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by a distinguished medievalist was a happy one. Dr. Pantin became keeper of the Archives in 1946 and over a period of more than twenty years presented the Delegates of Privileges with annual essays on various aspects of the history of Oxford demonstrating the utility of an old depository. Some of these essays or discourses have been assembled here. They light easily and briefly on a large number of themes and fly over nearly all the centuries of Oxford's history. There are discussions of admission records, lodgings, the growth of colleges, the evolution of the office of proctor, the chancellor's court; observations on academic freedom, on undergraduates, on the "pre-gerontocratic" history of university administration. The emphasis falls on the earlier centuries, but comments on more recent times certainly appear. Wherever Dr. Pantin goes he enlightens and makes his presence felt. And even when he touches upon subjects of special current interest like violence, student or otherwise, where jaded commentary is often the most anyone can manage, his remarks have a special effect. The jerky nightmare rhythms of the attacking town mob on a dreadful St. Scholastica's Day, 1355, which he has recovered from a fourteenth-century poem, pound in the head: "slay, slay, havock, havock; smite hard and give good knocks"!

The main reason for publishing these reports is to contribute to the present interest in the history of universities. While this interest is international Oxford has been consistently the most self-consciously historical of all universities, and Oxford dons and professors have been the most persistent in attempting to use their heritage and past to understand the changes of the present. The history of Oxford project recently started is but another example, although the most systematic, of this tendency, which includes the many volumes of the Oxford Historical Society, the Victoria County History of Oxford, the new series of occasional monographs from Christ Church College, and now Dr. Pantin's addition to the literature on the content of the University Archives. Besides describing some of the materials contained therein, he has also allowed us to see how they may be used, and the results are *inter alia* a charming pep talk. For it is no great challenge to produce journalistic accounts of our present discontents or argue in a time vacuum the purposes of higher education. But to reconstruct from basic sources the formidable complexity of past life is always daunting. It is, however, the only way to arrive at some finer appreciation of where universities now stand.

SHELDON ROTHBLATT
*University of California, Berkeley*

L. A. CLARKSON. *The Pre-Industrial Economy in England, 1500–1750*. New York: Schocken Books. 1972. Pp. 268. $10.00.

A survey of the early modern English economy has clearly been needed. This book has fulfilled that need, and on the whole successfully. The chronological framework, from the end of the Middle Ages to the eve of the Industrial Revolution, seems to me an easily defended choice. The title with its factual adjective "pre-industrial" indicates the lack of an overriding thesis concerning the period such as Charles Wilson chose in *England's Apprenticeship, 1603–1763* (1965), the closest previous approximation to this book. Clarkson does believe that Continental influences on England's industrial development were significant especially in establishing new industries.

He examines and largely dismisses an analogy between modern underdeveloped economies and the English early modern one as an anachronism. There is, however, a unifying theme. "The pre-industrial period is best understood as a market economy hampered by low productivity and poorly developed economic institutions" (p. 7), and there is reiterated emphasis on elements that constrained economic growth. The overall picture is that of an economy characterized by stagnation, yet changing patterns within the period are carefully elucidated. And there certainly was limited economic growth in some components of the economy and for restricted periods.

Within a disciplined compass the organization is topical rather than chronological. Scholars may rightly differ over the relative balance within subjects. There might have been more reference to the fishing industry, for example.

Although Wales, Scotland, and Ireland are virtually excluded from consideration, regional differences within England are emphasized. Indeed for some demographic topics, for example, only scattered local studies are presently available. Gaps in research are duly noted as

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are differing interpretations where no single opinion has received general acceptance.

To my view Clarkson does not use a satisfactory terminology for social classes. In terms of status he distinguishes between gentry and non-gentry, gentry to include the lay nobility. In another place in terms of landholding he speaks of "large secular landlords," which includes "titled aristocracy" (the lay nobility) as well as "non-titled aristocracy," an awkward equivalent to the county (as opposed to parish) gentry. Either of two alternatives seem to me preferable: a dichotomy of aristocracy—to include the nobility and county gentry—and the parish gentry or a threefold division into nobility, county gentry, and parish gentry.

I would also have valued more comparisons with contemporary economic conditions on the Continent. A few are made, but the viewpoint is predominantly insular.

The book is a well conceived summary of the present state of our knowledge, clearly, cautiously, and concisely written, enlivened by occasional flashes of humor, provided with an admirable bibliography of books and articles. Upper division and graduate students will find the book extremely useful in their studies; professors will find in it much material for their lectures. Clarkson's book will only be superseded when new research has been completed and new interpretations adumbrated.

H. S. REINMUTH, JR.
*University of Akron*

G. R. BATHO. *A Calendar of the Shrewsbury and Talbot Papers in Lambeth Palace Library and the College of Arms.* Volume 2, *Talbot Papers in the College of Arms.* (Historical Manuscripts Commission, JP 7.) London: H. M. Stationery Office; distrib. by Pendragon House, Redwood City, Calif. 1971. Pp. xix, 441. $40.00.

The physician-antiquarian Dr. Nathaniel Johnston rescued fifteen volumes of Talbot papers in the 1670s and had them deposited in the College of Arms. Other papers held by Johnston were transmitted after his death to the Lambeth Palace Library and were the subject of volume 1 of *A Calendar of Shrewsbury and Talbot Papers* (1966). This second volume, drawn from papers in the College of Arms, is much longer and is based on a selection of more than a third of the 6,404 folios in the fifteen volumes, which are designated by the letters A through P, omitting J. Although a number of scholars have made extensive use of these papers, they have here been systematically calendared for the first time. The fifty-eight pages of index naturally enhance the value of the collection.

These sources, apart from a few medieval and early Tudor items, are mainly concerned with the personal and public activities of three successive Talbots who were among the wealthiest nobles to serve the English monarchs from Henry VIII to James I. They were Francis, fifth earl of Shrewsbury (1500–60), George, sixth earl (1522–90), and Gilbert, seventh earl (1552–1616), and all were deemed worthy of significant notices in the *Dictionary of National Biography.*

The first four volumes of Talbot papers are mainly concerned with the military activities of Earl Francis, who became lord president of the Council of the North in 1549. He was greatly preoccupied with border warfare and encountered financial difficulties in maintaining his troops.

Volumes E through G deal with Earl George, who carried out functions similar to his father's, but the distinctive interest of these papers arises from his guardianship of Mary, Queen of Scots, from 1569 to 1584. He was quite unjustly suspected of favoring Mary's succession to the English throne, and it had to be explained that "unless she could transform herself to a flea or a mouse, it was impossible that she should escape." He was, moreover, very active in suppressing recusancy in the North and the Midlands.

His successor Earl Gilbert, however, (covered by volumes H through M) was lax in dealing with recusancy. This deficiency and his quarrelsome disposition account for the fact that the last of these Talbots "never enjoyed the full confidence of Elizabeth." The final three volumes of Talbot papers are miscellaneous in content and range in time from 1499 to 1615.

The volume in sum makes clear the manifold responsibilities of the county lieutenants.

WILLSON H. COATES
*University of Rochester*

LEONA ROSTENBERG. *The Minority Press & the English Crown: A Study in Repression, 1558–*

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*1625*. Nieuwkoop: B. de Graaf. 1971. Pp. xii, 263. 80 gls. ($22.00).

In her specialized monograph Rostenberg has gathered considerable material on the Catholic and Puritan printers of the time and has provided some idea of output and of the extreme difficulties under which these men labored. Unfortunately she has not clearly defined her subject, whether it be the influence of this minority press, the mechanics of its production and distribution, or a sort of mass biography of the shadowy figures of booksellers and agents. Consequently the promise of the introduction is unfulfilled, and the work remains a nonbook—an episodic collection of miscellany.

Her presuppositions are often unwarranted, and her occasional conjectures more embarrassing than provocative. Despite frequent footnoting, real documentation is wanting. "Undeterred by the force of the state, the Catholic minority and its champion, the underground press, remained courageous, steadfast and strong," is validated by "*Journals of the House of Commons*, p. 27," and "According to recent scholarship" followed by a citation to a 1910 publication. More serious is the author's not knowing what "seditious libel" meant in English common law or why libels against officials were always "seditious."

Too many questions are both unanswered and unasked. We do not learn the real significance of the press output of these minorities, what effect it had on various classes, the court, or the merchant or its influence on relations with The Hague or the Venetian Republic. Nor do we know the extent of circulation of these illicit works. It hardly satisfies to be told that "the imprints . . . made their way not merely to the modest homes of the English devout but also to the loftier residences of dignitaries and foreign plenipotentiaries."

Rostenberg obviously relied too heavily upon the scribblings of fanatical talebearers of the day. Their picture is distorted, for the times admitted of many competing informers eking out sordid lives on the fringes of a gullible and intolerant society, while much of the danger of the author's so-labeled "Great Catholic revival of the fifteen-eighties" undoubtedly existed only in the overheated heads of Elizabeth's advisers. Reliance on the minority printers themselves is equally risky, for though the author repeatedly portrays them as men of "faith and courage," as often they were disreputable hacks willing to print anything for a price.

The book is all the more difficult because of its nineteenth-century antiquarian style, replete with tidbits, diversional anecdotes, precious, archaic titles, and frequent lapses into meaningless rhetoric—"a burning taper held aloft, a votive candle to his God, a soaring flame of faith." Her concluding contention that the victory of seventeenth-century Puritanism stemmed from the activity of the minority press and the fact that their publications, unlike the Catholics', "bore no message of treason, no disloyalty to the Crown," even after two hundred pages, is still at most a dubious assumption.

ROBERT MUNTER
*California State University, San Diego*

R. C. RICHARDSON. *Puritanism in North-west England: A Regional Study of the Diocese of Chester to 1642*. [Totowa, N.J.:] Rowman and Littlefield. 1972. Pp. viii, 214. $10.00.

ERIC C. WALKER. *William Dell: Master Puritan*. Cambridge: W. Heffer and Sons. 1970. Pp. x, 238. £3.00.

Dr. R. C. Richardson provides a modest and unpretentious study of regional Puritanism, which, if it is not definitive, is nevertheless, like Richard Mather's preaching, "substantial and very judicious." The diocese of Chester was in some respects so atypical that the Puritan experience there can scarcely be considered representative—at the very time (1584) Whitgift was bending every effort to destroy Field's classical organization and clerical nonconformity in general, Bishop Chaderton set up fourteen preaching exercises at the urging of the Privy Council to illumine the "dark corners" of that benighted diocese. On the other hand, Richardson's contention that clerical participation in conventicles usually occurred where the Puritans found themselves an embattled minority within the parish and his suggestion that charges against a Puritan minister for inveighing against his parishioners usually disguised efforts to impose godly discipline from the pulpit—these and other observations and insights beg to be tested against the historical expe-

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rience of other English communities. In particular both Joan Thirsk's observation that Puritanism seemed to thrive best in certain agricultural communities where manorialism was weak, and Alan Everitt's thesis that large parishes with neglected communities within them were conducive to the growth of Puritanism seem borne out by Richardson's analysis of the Northwest, and these hypotheses about the sociological basis of Puritanism clearly need further investigation. Moreover this stimulating study leads one to wonder whether the impression that all Protestantism in the diocese had a decidedly Puritan cast was, first of all, true and, if so, whether such was due to the peculiarities of the diocese or whether the popular reformation throughout England, particularly among the laity, was largely a Puritan phenomenon. Finally, is the impression of a high degree of continuity in the Puritanism Richardson describes due to his analytic method, which de-emphasizes temporal change, or is it a fact that Puritanism as a popular movement, as distinct from the intellectualized Puritanism of the clerical controversialists, displayed much the same attributes from the middle years of Elizabeth's reign till the early 1640s?

Dr. Eric Walker's equally modest and unpretentious study of William Dell is ultimately disappointing as a biography because, through no fault of the biographer, so little that is personal—correspondence or anecdote—survives that Dell, the New Model Army preacher and later intruded master of Caius, remains an obscure figure, shadowy even to contemporaries who seem to have found him less charismatic than the tragic Saltmarsh and less forceful than the more politically astute Hugh Peter. But the work is doubly disappointing as history because, although Walker sets out dutifully to describe Dell's dozen-and-a-half published sermons and tracts, he tends to see Dell as an example of that perennial Christian figure and psychological type, the Enthusiast (Dell virtually disappears in his particularity in the final chapter, accurately titled "Religious Enthusiasm"). Hence he makes too little effort to discover Dell's individuality—to distinguish, for example, in what way Dell's view of the Spirit differed from Fox's conception of the Inner Light, or to explain why Dell, the hammer of Presbyterians and of all "people of the letter," should finally have come in early 1660 to accept, if not exactly to welcome, the prospect of Presbyterian success for the sake of the discipline promised by their ascendancy. Walker has usefully cleared up a number of obscurities about Dell's family and career, but he has been less successful in providing a coherent analysis of Dell's thought. The student who is less concerned with Dell than with seeking to understand the peculiar message of the army chaplains or the radical Puritan critique of learning and the universities might best still turn to Leo F. Solt's *Saints in Arms* (1959) and to Richard L. Greaves's recent *The Puritan Revolution and Educational Thought* (1969).

PAUL S. SEAVER
*Stanford University*

JOHN X. EVANS, editor. *The Works of Sir Roger Williams*. New York: Oxford University Press. 1972. Pp. cxlvi, 286, 5 plates. $25.50.

Sir Roger Williams's *The Actions of the Lowe Countries* (1618) appeared in an edition by D. W. Davies as recently as 1964. It appears now with the original spelling, extensive notes, a 133-page introduction, and a glossary. Most important, Evans has juxtaposed it with Williams's *A Briefe discourse of Warre* (1590). The two books, Williams's *Works,* may represent fragments of a larger book; certainly they belong together. Evans uses them to show Sir Roger as a highly talented soldier who devoted his life and thought to his vocation yet who reflects both the dawning spirit of empiricism and the court politics of his time.

Williams fought in Dutch, English, French, and Spanish armies in the period 1557–93, and Evans emphasizes Williams's reliance on experience as the basis for what he wrote. Yet, while *The Actions* covers the period 1567–74, about half of the book treats events that occurred before Williams came to the Netherlands. Indeed his account changes, becoming nearly straight campaign history, when he describes what happened after his own arrival on the scene. What were his sources of information for the early period? He reports conversations with William the Silent and others; what could Williams have read? What was the influence of Antonio Perez, who was like Williams a friend of the earl of Essex? What was the editorial role of John Hayward?

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Evans performs his editorial task ably, but his book points to further useful work, both closer analysis and investigation from a different vantage point. His own approach remains too English, whereas Williams's primary interest lies in what he has to say about Continental affairs. For instance, the *Briefe discourse* is largely concerned with describing the Spanish army. Evans uses the great collections of correspondence and the major histories of the period, and he uses well Bernardino de Mendoza's work on the army. Otherwise, Evans does not compare Williams's book carefully enough with Continental accounts of the army of Philip II from the sixteenth century and later.

PAUL STEWART
*Southern Connecticut State College*

JAMES M. OSBORN. *Young Philip Sidney, 1572–1577*. New Haven: Yale University Press for the Elizabethan Club. 1972. Pp. xxiv, 565. $25.00.

This book is a vastly detailed reconstruction of the travels and correspondences of Philip Sidney from the time he left England for his Grand Tour in May 1572 until the conclusion of his embassy to the Holy Roman Empire, the Palatinate, and the Netherlands in May 1577. The terminal dates were chosen in order to encompass the group of sixty-five hitherto unpublished letters to Sidney purchased by the author at Sotheby's in 1967 and deposited by him at Yale. These, with the thirty-odd other items of previously unused Sidney correspondence in other collections and the letters already known to students, especially those of Hubert Languet, furnish not only the occasion but the focal point of the volume: most are quoted in their entirety and sometimes commented upon at some length in the text. The emphasis upon Sidney's correspondence is so great that what is produced is not a biography at all but a commentary on the letters, enlarged by discussion of the political and cultural contexts in which they were written and received.

Given the focus Dr. Osborn has chosen, the value of the book must rest primarily on what is contained in the newly discovered letters—which is, unfortunately, not nearly enough to justify this inflated (but handsomely produced) edition. Too many of the letters are the formal stylistic exercises in which the humanist scholars indulged, composed very largely of exaggerated compliments to the recipient. Many more are newsletters, in which it is easy to trace the fears and hopes of Sidney's friends over political events in France, Poland, and the Empire. But few indeed can be said to illuminate significantly any aspect of Sidney's mind during his "formative" years, which is Dr. Osborn's expressed aim. The letters describe political occurrences real and rumored, and Osborn in his text describes the persons writing the letters, the places Sidney visited and the events he might have witnessed, but in the midst of it all Sidney himself remains a shadow, constantly praised but never revealed. The assumption is clearly made that if we know where Sidney was, what he was looking at, and what was in his mail, then we shall know Sidney, but the assumption is palpably false and even does Sidney an injustice, which is the last thing the author would wish.

Osborn's admiration for his subject is so intense that any standards of critical scholarship are quite overcome. The advanced Protestant attitude toward foreign policy, for example, was the correct attitude because Sidney and his friends shared it, and Queen Elizabeth and Lord Burghley "rationalized instead of thinking" because they didn't. Elizabeth did not give Sidney a major appointment (assuming that an embassy to Vienna and Heidelburg was not honor enough for a youth of twenty-two) because she feared his talent and hated his honesty, not because she had an impossible task in satisfying the demands of a court hungry for preferment. The absence of any real historical perspective or any real understanding of the Elizabethan court reinforces the reader's wish that Dr. Osborn had been content with publishing his new letters in a slender, modest volume for the Sidney specialist. This more ambitious undertaking was a mistake.

ROBERT W. KENNY
*George Washington University*

LELAND H. CARLSON, editor. *The Writings of John Greenwood and Henry Barrow, 1591–1593*. (Sir Halley Stewart Trust Publications. Elizabethan Noncomformist Texts, Volume 6.) London: George Allen and Unwin, for the Trust. 1970. Pp. xiv, 516. £6.50.

Historians have long recognized the signifi-

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cance of the Separatist movement of the late sixteenth century for the political and religious development of both England and America, but until the publication of this series historians have lacked adequate texts for a full appreciation of Separatist thought. Dr. Carlson has already provided readers with the complete works of Robert Browne and Robert Harrison, two of the founders of the movement, together with some documents from Thomas Cartwright's career that are relevant to the Separatists. With this volume Dr. Carlson brings to completion the publication of the manuscript and printed works of the clergyman John Greenwood and the lawyer Henry Barrow, who continued their literary warfare against the Established Church from their prison cells. Greenwood, in his *Breife Refutation of Mr. George Giffard His Supposed Consimilitude betwene the Donatists and Us* (1591), has little difficulty in showing that the Separatists were not schismatics on the Donatist model since they did not accept the ministry, worship, discipline, or polity of the Established Church. He rather adroitly argues that it is the Reformist or Puritan movement, with which his opponent Gifford was associated, that came closer to ancient Donatism. Barrow, in his hard-hitting *Few Observations to the Reader of Mr. Giffard His Last Replie* (1592), makes it clear just how complete was the Separatist rejection of the practices of the Church of England. Included among that church's objectionable features were its "whole publick worship and administration," its financial arrangements, its liturgical calendar and lectionary, and, especially, its reliance upon civil sanctions to enforce religious uniformity. These ideas were long to reverberate in English-speaking lands.

As irritating as the Separatists must have been to those in authority, the treatment accorded them by the judicial officials of Elizabeth's government does not reflect much credit on the much-admired Elizabethan settlement of religion. Barrow's complaint in his "Letter to an Honorable Lady and Countesse of His Kin[d]red" (1593), written shortly before his execution, that he has been falsely condemned for "moving sedition and disobedience" against the queen seems not unfounded. The Separatists, as the large number of legal examinations included in this volume indicate, were almost pathetically loyal to the queen and the civil government, though they had endured hard usage, indeed, for their refusal to conform in religious matters. The petition of April 1593 to the magistrates, probably written by Francis Johnson, is an effective challenge to the policy of persecution then being pursued: "As for dungeons, irons, close ymprisonment, honger, cold, want of meanes to mainteyne their famulies, theise may cause some to make shipwrack of a good conscience or to lose their life, but they are not fitt wayes to perswade honest men to anie truth or diswade them from errors" (p. 423).

Dr. Carlson has skillfully edited these final writings of Greenwood and Barrow, together with the legal and other documents relating to the imprisonment of Separatists in the early 1590s. In the six volumes published to date he has provided a wealth of bibliographical and historical information about a movement that is much less well known than that of Presbyterian Puritanism. It is hoped that in a future volume some connected account of Elizabethan Separatism may be provided to ease the lot of those who will otherwise have to piece the story together from editorial materials and a myriad of footnotes.

W. B. PATTERSON
*Davidson College*

W. J. JONES. *Politics and the Bench: The Judges and the Origins of the English Civil War.* (Historical Problems: Studies and Documents, 13.) New York: Barnes and Noble. 1971. Pp. x, 14–228. $10.50.

Professor Jones has chosen a most important topic for his contribution to the Elton series of Studies and Documents. The problem is that the topic is too important for such a cursory treatment—there are fewer than 140 pages of text to underpin the documents. It is also unfortunate that Professor Jones chose to present his material in episodic fashion. There is a theme, and a most important one, but the beginning student, for whom the series is designed, will have a difficult task in keeping his eye on the main issues.

Several high court judges were impeached in the early days of the Long Parliament. Why? That is the question this book attempts to answer, or at least it attempts to provide materi-

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als from which an answer might be drawn. The law and the bench were caught in the middle of a political struggle for power between the Crown and Parliament. The judges were "the lions under the throne" and the members of Parliament were all deeply imbued with reverence for the common law of England. Each side looked to the judges for an ally and for final proof of the righteousness of its cause. It was impossible for the bench to satisfy both, and, indeed, it was difficult enough to be true to itself.

James I and Charles I came increasingly to tax and to rule by consent of the judges. Parliament in turn looked to them to restore the ancient and fundamental law. The judges were answerable to the king for their conduct and, in any case, the king often had the letter if not the spirit of the law on his side. When the Long Parliament finally convened, it was not yet possible for the two Houses to attack the law or the Crown openly, so the judges and the royal advisers, such as Strafford and Laud, had to be the scapegoats. The point had been reached when the king and the judges could no longer use the letter of the law to justify acts that met with the overwhelming political opposition of the nation.

One hopes that Professor Jones will someday write a more comprehensive account of this struggle for the rule of law and will link it with the attacks upon the common law itself that grew out of the civil war of the 1640s.

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*The Diary of Thomas Isham of Lamport (1658–81), Kept by Him in Latin from 1671 to 1673 at His Father's Command.* Translated from the original by NORMAN MARLOW, with introduction, appendixes and notes by SIR GYLES ISHAM, together with a preface by SIR GEORGE CLARK. [Farnborough, Hants.:] Gregg International Publishers. 1971. Pp. 343.

Whatever the reaction of historians to this handsome quarto, antiquarians will delight in it, chiefly because of the scaffolding that envelops the two versions of the diary. In the English rendering, not a name, not a place lacks its footnote, many of them in fine print hundreds of words long. For the Latin original, the notes, far fewer in number but still extensive, are largely philological. In his introduction Sir Gyles describes with equal loving care the circumstance of the diary, the characters of Thomas's father, Sir Justinian, and several persons who appear frequently, the domestic arrangements of Lamport Hall, and the short and unsatisfactory later life of Thomas. Although Sir Gyles believes that materials for a full biography exist, neither the evidence he supplies nor the diary itself are enticing. Thomas seldom comes to life. The entries in the diary are mostly brief, leading one to suspect that Thomas scamped his Latin chore. Later on he seems to have been a "cardboard lover," for none of his planned alliances came off, which suggests deficiencies never spelled out. The one prospect that might have come to fruition was terminated by his death. The moralizing from his contemporaries and from the editor about the "ruinous effects of a life of Plesure" and what might have been had Thomas married happily and so exchanged the "vices" of Restoration London for the virtues of bucolic Lamport only hints at the "indefinable" charm that "attracted very diverse types."

To say that the diary contains little promise of future interest is not to dismiss it cavalierly. Students of Restoration England will discover informing tidbits—sports and pastimes, daily coming and going, the persistence of witchcraft and its horrid penalties, the Christmas dinner for the local poor, clues as to wages and prices, and enthusiasm for gadgetry manifest in the reference to Sir Samuel Morland's "stentorophone." Prominent is the steady recording of sermons, marriages, pregnancies, births, and, above all, deaths, deaths by accident or disease; far more people die than are born, or so it seems. Perhaps most informing of all is the attention to the Dutch war, French activity, and the Turkish threat; there is a good deal of rumor but plenty of fact, too. In sum, the diary discloses what one might expect from the tolerably intelligent teen-age son of a wealthy provincial family.

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J. R. WESTERN. *Monarchy and Revolution: The English State in the 1680s.* Totowa, N.J.:

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Rowman and Littlefield. 1972. Pp. viii, 421. $11.00.

The late Professor Western's *Monarchy and Revolution* is not a narrative history of the 1680s in England but a series of essays. After a long essay on the political theory of the period Western turns to the ways in which Charles II and James II "refurbished the monarchy" far more successfully than is generally recognized. There follows an account of the "advent of William of Orange" and essays on the results of the successful expedition of 1688. Western uses a wide range of secondary and printed sources, but little manuscript material. The result is a convenient and frequently enlightening synthesis of recent interpretations of the period.

The author of a good monograph on the eighteenth-century militia, Western knows military history better than other fields. His description of the rehabilitation of the army and navy under Charles II and James II is extremely good. He is a traditionalist in his treatment of the revolution of 1688. He follows William III's recent biographer, Stephen Baxter, in judging the descent on England to have been inspired not by personal ambition for the English throne but by William's need for a solid English alliance against Louis XIV. To credit William with so little personal ambition seems forcing the evidence. Western is much more convincing in pointing out how extremely risky the venture was from the military point of view.

Western is also traditionalist in viewing the revolution of 1688 as a conservative revolution, but he adduces plenty of solid evidence in the last third of his book, demonstrating how far short of its goals the revolution proved to be. Limitations on the monarchy were effected in small installments over a long period. The guarantee of Parliament's power of the purse was the result of circumstance—the continuing state of war—that made annual sessions obligatory for voting the necessary funds. As for religious toleration, the High Church party and the Presbyterians could not agree on a "comprehensive" church; so the Anglicans kept the traditional Established Church but lost the weapon of persecution. The Dissenters, whether Protestant or Catholic, won the legal right to practice their religion but labored under penal legislation: the Corporation and Test Acts.

As an up-to-date and convenient treatment of the main issues of the 1680s and the Glorious Revolution Western's work is commendable. It is stronger on military, administrative, and financial affairs, but religion and political theory get adequate space. Students of later Stuart England can be grateful that Western's widow and friends made possible the publication of this posthumous work.

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*Calendar of State Papers Preserved in the Public Record Office.* Domestic Series. *James II.* Volume 3, *June, 1687–February, 1689.* London: H. M. Stationery Office; distrib. by Pendragon House, Redwood City, Calif. 1972. Pp. ix, 523. $50.00.

The most important of the 2,395 items calendared here have long been in print. It is a pity that the documents made available by Sir John Dalrymple in his *Memoirs of Great Britain and Ireland* (1771, 1773) could not have been reprinted for this occasion, for the *Memoirs* is not well arranged nor is it always at hand. Of the significant documents for the revolution, only the invitation of the "Immortal Seven" of June 30, 1688, is reproduced textually. For all the others the student is referred to Dalrymple or the works of Japikse, Cobbett, Bloxam, and so on. But we must be grateful for what we have. There is the usual mass of papers on local matters and Irish affairs, which will be useful for those interested in the remodeling of the corporations or in the practical effects of the Declarations of Indulgence. The most interesting material concerns the defensive measures taken by the government in 1688 as it belatedly awoke to the threat of invasion. The unpopularity of James II's administration is clearly shown by the refusal of so many gentlemen to act as militia officers. In a work of this sort, of course, the vital question is the quality and thoroughness of the index. This one, compiled by Mr. A. J. W. McDonald, is admirable, and it occupies no less than 100 pages, as compared to 420 pages of text.

These papers indicate that James II had some real claim to be regarded as a tolerant ruler. His protection was extended to Jews,

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Huguenots, Calvinists, Quakers, and Congregationalists as well as to his co-religionists. The use of Roman Catholics in public employments, however, was a very different matter in the eyes of contemporaries. That was an insufferable attack on the social order, as mere toleration might not have been. When the king spoke of his government as "mild" he meant, presumably, that there was peace and prosperity as well as toleration. He could also have been referring to his generous record in pardoning those convicted of capital crimes. But the weakness of his foreign policy and the threat his domestic ideas seemed to pose to the Church of England interest more than offset his good intentions, and the lighthearted way in which James II regarded the law was just too much. Had he been more patient, had he been more careful of the property and of the religious susceptibilities of his subjects, James II might have gone far: as it was, he went to St. Germain.

Surely it would be appropriate for the government of this country to assist in carrying this great series forward at least as far as 1783.

STEPHEN B. BAXTER
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RICHARD S. TOMPSON. *Classics or Charity? The Dilemma of the 18th Century Grammar School.* [Manchester:] Manchester University Press. 1971. Pp. viii, 168. £2.76.

This book will not be to the taste of those who prefer form to substance in their historical reading for it is a thorough, skillful quantitative inquiry into the eighteenth-century English grammar school. The author's task was a difficult one because the material for such a study is widely scattered, and for decades historians have been satisfied with misleading generalizations about the "decadent" character of the schools. Dr. Tompson has overcome these difficulties by amassing data on 334 grammar schools and by analyzing these data in a sensible and unpretentious fashion. His conclusions compel a considerable revision of the generally accepted condemnations of the institutions.

Dr. Tompson writes clearly, although far from elegantly. His intellectual mien is a trifle timid, so the book is not as stimulating as it might have been. There is, it appears, one serious editorial slip-up (p. 127), but otherwise the work is free from major errors. Manchester University Press has produced the book handsomely with footnotes at the bottom of each page, where they belong.

DONALD H. AKENSON
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GORDON JACKSON. *Hull in the Eighteenth Century: A Study in Economic and Social History.* New York: Oxford University Press for the University of Hull. 1972. Pp. xvi, 474. $27.25.

R. G. WILSON. *Gentlemen Merchants: The Merchant Community in Leeds, 1700–1830.* [Manchester:] Manchester University Press; New York: Augustus M. Kelley. 1971. Pp. viii, 271. £3.60.

One of the key areas in the first phase of British industrialization was the Yorkshire textile region. Within the West Riding, Leeds was the main manufacturing and financial center while Hull was the major outlet. On the side of exports the major factor was the growth of cloth exports from between one hundred thousand and two hundred thousand pieces at the beginning of the eighteenth century to four hundred thousand pieces at the end. By 1795 Hull was also exporting Manchester cottons to northwestern Europe. It was also the major outlet for the hardware products of South Yorkshire, for the pottery of the Midlands, and, particularly in the middle of the century, for Derbyshire lead. Iron, timber, flax, and hemp from Scandinavia and the Baltic and a variety of products from Germany and Holland were the main imports. Since the import trade consisted largely of raw materials, it was larger in volume but smaller in value than the export trade. While the greater part of Hull's trade was with northern Europe, some wine came in from southern Europe while tobacco, some of which was re-exported, came from America. Hull also had an important stake for a period in the whaling trade and a considerable coastwise trade, particularly with London. Although Dr. Jackson may underrate the importance of Hull at the beginning of the century, the growth of these trades made Hull the fourth most important port in England by 1800. This account of the trade of Hull occupies almost a third of the text of Dr. Jackson's volume, and

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almost all the appendixes—which with the lavishness of a university press themselves amount to over one hundred pages and include a forty-page transcription of the records of the coastal trade of Hull in 1775—illustrate this story. But Dr. Jackson is also concerned with the growth of Hull's shipping fleet and the effects of war on it, with the emergence of the shipowner as a distinct occupation, and with the development of marine insurance. Alongside shipbuilding and related trades, oilseed crushing, paint manufacture, sugar refining, and a range of miscellaneous industries were carried on there. But there was no clear-cut distinction between trade and industry and most of the industrialists were merchants. As in other commercial centers the need for credit and a simple system of payments led to the foundation of banks, all but one of which were linked with a major merchant house. And as in other British ports, port improvement became a contentious necessity before the end of the century.

A further theme with which Dr. Jackson is concerned and to which Dr. Wilson devotes his whole book is the nature of the business community. In the recruitment of merchants there were similarities between Hull and Leeds. In both towns the origins of the majority of merchants were not to be found among merchants but among the gentry. Out of ninety-three merchant apprentices in Hull between 1720 and 1800, thirty-two were "alien" gentry and eleven were Hull gentry, while in Leeds recruits came most extensively from the local gentry who were wealthy enough to pay the apprenticeship dues. Then, as Professor Willan has shown for mid-sixteenth-century London and contrary to a common view of upward social mobility, while a few merchants acquired enough wealth to buy land, most of them stayed in trade. The majority of Leeds merchants, Dr. Wilson states, never purchased a landed estate while Dr. Jackson hazards "a tentative conclusion that few Hull merchants moved from staith to estate *in the eighteenth century*." In both towns, while dissenters were both numerous and influential, Anglicans were in the majority though the torpor of the Anglican Church in both Hull and Leeds was shaken by the Methodist and Evangelical movements. Then merchants alike in Leeds and Hull had little taste for the classical education of the grammar schools, and so in both towns new schools were founded to provide a more appropriate education for those intending to enter the world of business.

While Dr. Jackson stops his account about 1800, Dr. Wilson continues his story for a further thirty vital years during which an important change in the marketing of cloth in the West Riding of Yorkshire took place. The French Revolution and the Napoleonic wars disrupted traditional markets at the end of the century, the rise of the factory transformed the manufacture of cloth, while the dictates of fashion led to a shift in demand from broadcloth to worsteds. So Yorkshire woolen manufacturers came to market their own cloth. As a result the number of Leeds woolen merchants fell from the peak figure of 150 in 1800, to 100 in 1830, and to about 70 by 1851. And with declining economic importance came the end of the long monopoly the Leeds woolen merchants had held of the city corporation and of public office. The day of the gentleman clothier was over.

Both these books indicate that distribution as well as manufacture have an important place in economic development. Dr. Wilson has provided an important study of a major merchant group while Dr. Jackson has written a thorough account of the history of a major English port. Both are to be welcomed and deserve emulation for other manufacturing centers and ports.

W. E. MINCHINTON
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J. T. WARD and R. G. WILSON, editors. *Land and Industry: The Landed Estate and the Industrial Revolution. A Symposium.* New York: Barnes and Noble. 1971. Pp. 280. $12.00.

The idea that landed and industrial interests were in fundamental conflict during the classical period of Britain's industrialization is a discredited myth that yet endures in the absence of a new synthesis of the relations between land and industry. Hence a collection of essays devoted to explaining the vital contribution of the landed interest to the development of industry could be very valuable.

Landowners became heavily committed to industry almost from the start for, as J. T. Ward notes in his essay "Landowners and Min-

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ing," it was natural that the holders of the largest proportion of the nation's territory be in a position to provide a large proportion of the capital for the development of extractive industries, their infrastructure, and the urban land and building for housing the new population of industry. But as David Spring ("English Landowners and Nineteenth-Century Industrialism"), R. W. Sturgess ("Landowners, Mining and Urban Development in Nineteenth-Century Staffordshire"), and W. H. Crawford ("Ulster Landowners and the Linen Industry") all make clear, the impact of the commitment on the landed interest was an ambiguous one. It provided both an opportunity and a threat: an opportunity to renew and to enormously multiply landed wealth and a threat to the established structure of landed society. Throughout the nineteenth century this ambiguity remained, though toward the end the involvement, the profits, and the relative social position of the landowners all declined, the first two gradually, the last rapidly. On the other side of the coin (according to R. G. Wilson in "The Denisons and Milneses: Eighteenth-Century Merchant Landowners" and T. M. Devine in "Glasgow Colonial Merchants and Land, 1770–1815"), merchants who bought into land for purposes of social mobility were among the most effective "improvers" and saw to it that their snobbery paid good returns, though admittedly the illiquid nature of the investment from time to time hastened hard-pressed mercantile fortunes into bankruptcy.

Unfortunately, though such enlightening themes may be mined from this symposium, it fails to provide a useful general conceptual framework. Save in the case of T. M. Devine's essay, there is no attempt to support generalizations and judgments with quantification or to suggest how representative of the landed industrialists as a group were the isolated figures discussed. Several of the essays are, at one and the same time, cluttered with the detail of pedestrian scholarship and of insufficient depth to evoke new interpretations. The elements of the new synthesis have still to be defined.

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JAMES A. HENRETTA. *"Salutary Neglect": Colonial Administration under the Duke of Newcastle.* Princeton: Princeton University Press. 1972. Pp. xii, 381. $14.50.

This is a work of exceptional character, possessed of some outstanding qualities. From its finely measured beginnings in the politically undistinguished time of the Hanoverian succession it moves to a broadened perspective in the years after the mid-century. There is here a feeling for empire that few contemporaries can better and an understanding of politics that adds much to the important essays on the Anglo-American connection recently edited by Professors Alison G. Olson and Richard M. Brown. James Henretta is not emotionally involved with Newcastle, and if Henretta's mechanistic assessments of the man possess a chilling quality, he nevertheless shows more respect for the duke than many in the past and is not concerned to attack him over trivialities.

This study is firmly based on intensive research. Its importance in illuminating the darkest ages of American colonial history can scarcely be underestimated. Its best features relate to the 1740s and onward, and the closing chapters have more controlled power than anything that has been written for some years on the politics of this period. However, for all its merit, this work is not without weakness. Henretta's view of Newcastle's attitude to office before he had gained full power is ambiguous. Between 1724 and 1730, as the author concedes, the duke brought enthusiasm and drive to the post of secretary of state, resolving to uphold the highest standards of administration. Thereafter, until near the end of the thirties, his interest in colonial affairs fell away. Henretta repudiates my claim (p. 134) that before 1739 Newcastle was unsystematic in his use of the power of patronage, yet the author subsequently advances a good many statements to support this view. He admits that the process was "more haphazard and absent-minded than purposeful and determined" (p. 246) and that until 1741 Newcastle "acted at the instigation of others and for their benefit" (p. 253). It would seem that the opinion Henretta dismisses with such flourish has been more influential than he is willing to allow. Acknowledgment of some of the clearings that have eased his path could not have modified significantly the considerable success in turning aside from false trails.

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It was not my contention that Newcastle pursued a constructive imperial policy: I considered that he bore more than a fair share of responsibility for its failures. The title *Salutary Neglect* is, of course, ironic. The main consequence of the Pelhams' engrossment of patronage was the critical weakening of executive power in the colonies. For this, more particularly after 1742, Newcastle must take a major share of the blame. It is retrograde, however, to speak of the growing belief in parliamentary sovereignty, of which the duke disapproved, without balancing reference to its intellectual foundations and the influence of other contemporary agents of transmission. To do otherwise is to raise him too high. In some of the "iffy" speculations arising from the crucial clash of opposing administrative traditions there appears the conventional figure of the scapegoat burdened with the failures of an age.

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OWEN CHADWICK. *The Victorian Church*. Part 1, *1829–1859;* part 2, *1860–1901*. (An Ecclesiastical History of England, Volumes 7–8.) New York: Oxford University Press. 1966, 1970. Pp. x, 606; viii, 510.

These two volumes, massive and masterly, are a landmark in English religious historiography: the best (one is tempted to say, the only) historically sound and comprehensive survey of nineteenth-century Church history. What Norman Sykes was to the eighteenth century, Owen Chadwick is to the nineteenth; and the wide range of his works establishes him as the leading ecclesiastical historian of our times. His *Victorian Church* is not the history of an institution but rather a detailed portrayal of the religious life of the nation in its political, social, and intellectual dimensions. The "Church" is broadly understood as the Christianity of England, including Protestant Dissenters and Roman Catholics as well as the inevitable emphasis on the Church of England; it excludes, however, Scotland, Ireland, and the missions. The inexplicable accident that the first volume was not reviewed here on its appearance in 1966 allows us now to celebrate the publication of the second by considering the work as a whole, the better to recognize its achievements and, perhaps, its limitations.

Chadwick begins in 1829 with Catholic emancipation, which together with relief for Dissenters in 1828 marked the point at which the Church of England, though still established, could no longer rely on an identity of interest with the state. There began a period of political and ecclesiastical readjustment to the free competition of religions, as well as to the rapid changes in the conditions of society. Chadwick divides his two volumes in 1859, when the publication of Darwin's *Origin of Species* symbolized the beginning of a phase of intellectual doubt that challenged basic assumptions hitherto common to all the competing factions and sects; however, he rightly regards Darwinism itself as of only minor importance as a challenge to faith. The concluding date of 1901, though inevitable, has no particular significance.

The first volume covers its thirty years in largely chronological fashion, taking each crisis or controversy in its turn, examining it closely, setting it in its place, and moving on. The sketches are insightful, the judgments are magisterial, and the treatment is overpowering in its detail: if it isn't in Chadwick, it didn't happen. Although there are a few analytical sections, there is virtually no generalization or large-scale conclusion. What conclusions Chadwick offers are as trenchant as they are terse: a brilliant introductory statement of the problem (pp. 1–6) and a few paragraphs of useful summary (pp. 568–72).

The second volume is surprisingly different. Almost disregarding chronology, Chadwick organizes his material into analytical chapters, dealing with large intellectual and social topics. The intellectual problems (science, Biblical criticism, and doubt) occupy his attention first, but the most impressive feature of the volume is its treatment of social questions. The chapter on "The Town Church," which compelled the admiration of K. S. Inglis, is a remarkable achievement, especially for one not trained in the special discipline of social history. The chapter on "The Village Church" is equally excellent. If Chadwick's conclusions are somewhat tentative, they are as much as the state of the art presently allows, and they leave no doubt that social history is the direction that most future religious historiography should take. In the various chapters of his second vol-

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ume, Chadwick is more willing to draw conclusions than in volume 1; but there is no summary or general conclusion at all.

The glory of the work, aside from its thorough comprehensiveness, lies in the sudden insights that spring up everywhere in the two volumes, especially in the delightful biographical sketches. Chadwick has the knack of portraying a character, sometimes in a paragraph, sometimes in only a few words, always with telling effect. Sometimes, indeed, one feels that he is almost too intimate with his characters; his affection for Archbishop Howley seems more than merely historical. He has a sure touch in selecting the decisive detail, the insignificant fact that illuminates many other facts, whether it be the abandonment of smocks by agricultural laborers in the 1860s or the reading-lists of a clerical library in Lincolnshire. In such moments of insight lies the delight of reading the work.

Chadwick has expended his most complete mastery of the subject in the accumulation of such details, producing a picture of the Victorian church that is more a mosaic than a photograph, somewhat uncertain in its outlines but incredibly rich in its texture. He is a practitioner and a master of that art of historical writing that conceals analysis behind the form of narrative and a mass of details. His style is crisp and lively, though some American readers may find it difficult because of its allusiveness and its irregular paragraph structure.

Some may regret the absence of larger conclusions and generalizations. There is a central problem in the historiography of nineteenth-century religion that must be faced by any comprehensive study. On the one hand it was a century of impressive religious revival, which especially in England pervaded the life of the nation. On the other hand, the position of religion had receded from the height of its revival by the end of the century; it had lost its centrality and, perhaps, was becoming irrelevant. There seems to be a gap between effort expended and results achieved, and the historian must try to deal with this. Does he discover, with Desmond Bowen, the growing realization of *The Idea of the Victorian Church* (an "idea" mercifully veiled from Chadwick), or does he find, with Peter Marsh (Chadwick's student), *The Victorian Church in Decline* already in the 1870s? Chadwick, after supplying so many of the elements for a decision, does not after all decide, although he tends to minimize the decline of religion in the last third of the century.

In general, Chadwick seems inclined to a rather optimistic view of the situation, an optimism that is surprising. This may be more than a matter of taste. Should one be satisfied that as many as thirty-eight per cent of the people attended church in 1881, or dissatisfied that as few as thirty-eight per cent attended? Chadwick, on the side of the angels, is satisfied. Though he acknowledges a decline after 1886, his final chapter on "Secularisation" is inconclusive and ends rather incongruously with a discussion of the varieties of Victorian devotion. Perhaps the faith of the Master of Selwyn College rules the judgment of the Regius Professor of Modern History.

In another sense, Chadwick's reluctance to draw firm conclusions may be justified. One measure of the greatness of his work is that it shows us not merely what is known but how much we do not know. This is especially the case in social history, though even in intellectual history (particularly when dealing with Biblical criticism) Chadwick is occasionally forced to break new ground. It is also the case with the non-Anglican denominations. Chadwick falls into the common trap of devoting fully half of his chapter on the Roman Catholics to Newman, although he gives a good treatment of the Irish immigrants in the chapter on "The Town Church." His examination of Nonconformity, largely confined to a chapter in volume 1, does not come to grips with the heyday of the "Nonconformist conscience." Here, however, he may justly plead the paucity of monographic studies. And can anybody but a Welshman understand the Calvinistic Methodists?

*The Victorian Church*, then, is not definitive. It is, nonetheless, a major achievement, the standard by which other works must be judged, and the starting point for further research. For the next twenty years, we will all be writing footnotes to Chadwick.

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GLEN PETRIE. *A Singular Iniquity: The Campaigns of Josephine Butler.* New York: Viking Press. 1971. Pp. 317. $7.95.

When one considers the amount of recent scholarly interest in the question of Victorian sexuality and the fact that Glen Petrie's book is, in effect, the third biography of Josephine Butler in less than two decades, one can no longer regard her as an unsung Victorian heroine. Although Petrie devotes attention to all of her various campaigns—in favor of women's education, for individual "rescue work" akin to Gladstone's, against "regulated vice" on the Continent, and in support of W. T. Stead's drive against "white slavery" and for raising the legal age of consent for girls—the author's main focus remains on Josephine Butler's religiously inspired and ultimately successful campaign against Victorian Britain's prime experiment with government-regulated prostitution, a system that existed from the mid-1860s to the mid-1880s in twelve communities adjoining army camps. Josephine Butler remains a fascinatingly individual feminist, courageous in her public avowal of the humanity of prostitutes and deeply devoted to the eradication of the double standard in sexual morality but equally willing to use her "feminine wiles" to achieve her ends. She was herself a partner in a felicitously successful marriage, and the cause of women's suffrage left her cold.

Mr. Petrie has written a highly readable but not a particularly profound book. His sources are largely confined to the multitudinous writings, some hitherto unpublished, of Josephine Butler and her circle, and he seems scarcely ever to have bothered to look up for himself a reference to a newspaper or to a parliamentary debate. The problems of nineteenth-century England are rather too likely to be discovered by him during precisely that decade in which his heroine discovered them for herself. Josephine Butler was the daughter of a first cousin of Lord Grey of Reform Bill fame, and the Liberals remain, by and large, the author's heroes and the Conservatives his villains. Mr. Petrie thus seems to fail to appreciate the ironies implicit in the fact that Liberals were responsible both for the passage of the Contagious Diseases Acts, which established compulsory medical examinations for prostitutes, and for their subsequent repeal and that their passage can, in defiance of conventional chronology, be interpreted as a Victorian foundation stone of the national health service and their demise as a belated triumph for laissez-faire and financial retrenchment. But perhaps for the author, as generally it was for Josephine Butler, the subject is too serious to merit irony.

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J. B. CONACHER. *The Peelites and the Party System, 1846–52.* [Hamden, Conn.:] Archon Books. 1972. Pp. 246. $10.00.

WILBUR DEVEREUX JONES and ARVEL B. ERICKSON. *The Peelites, 1846–1857.* [Columbus:] Ohio State University Press. 1972. Pp. xii, 259. $10.00.

It is convenient for readers, if perhaps not for the authors, that two books so closely related in theme as these should appear simultaneously. This circumstance allows for instructive comparison between them, and, insofar as their independent approaches leave problems unresolved, they may indicate the limits to which the discussion can be carried.

The Peeliters were the makeweight in the political balance after 1846. They included the cream of political talent from the Tories, and they outshone most of the figures among the Whig-Liberals. Their shifts in support made and broke governments for nearly fifteen years, and no ministry could be sure of its stability so long as they remained outside the party system. Yet as a group they have always been ill-defined and obscure. They rallied round Peel until his death and both then and after they usually could be counted on to support certain issues—free trade, religious toleration, a moderate foreign policy, efficiency in government administration. But in "the golden age of the independent member" they were a volatile element. They lacked leadership and organization and were not sufficiently coherent to advance a definite group of policies as a party. Instead they made up an alluring bevy of votes that successive party leaders wooed until Palmerston finally captured them for the Liberals in 1859.

Both these books adopt a narrative approach, tracing the actions of the Peelites in Parliament through the years that followed repeal of

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the Corn Laws. Their research, although not based on identical sources, depends largely on overlapping ones, including the papers and biographies of the leading figures among the group. Curiously, neither makes use of contemporary periodical literature, although the reviews contained continuous comment on the state of political parties. Both works attempt to clarify the identity of the Peelites. Conacher sees them as followers of Peel who were Free-Trade Conservatives and who faced each issue rationally as it came. Jones and Erickson base their definition more strictly on those who voted with Peel in 1846, but they find them supporting similar issues in the years that followed. The authors have devoted enormous effort to tabulating the Peelite votes, the results of which they present in either the text or appendixes.

Yet the books diverge significantly. Professor Conacher's work is one installment of a three-part study of the Peelites from 1846 to 1859, of which his magisterial *The Aberdeen Coalition* forms the second stage. Here, in a study that grows out of an earlier article, his concern is with the group from Peel's fall to the formation of the Aberdeen coalition in 1852. He argues that the Peelites floundered without leadership or organization until Peel's death. The possibility of fusion with one or another of the traditional parties was always present after 1850 but was thwarted just when it seemed imminent by issues that divided the Peelites from their intended partners. The Don Pacifico incident, the Irish Franchise Bill, and the Ecclesiastical Titles Bill alternatively swung them back and forth from the Whigs to the Tories. Conacher believes Gladstone's attack on Disraeli's budget of 1852 was critical in turning the Peelites back toward eventual fusion with the Whig-Liberals, a union in which the Aberdeen coalition played a necessary preparatory role, only to have the final juncture postponed by the Crimean War and Palmerston's war ministry until the end of the 1850s.

Professors Jones and Erickson deal with a longer period than Conacher. They begin with an extended discussion of the crisis in Peel's cabinet in 1845 and carry the story to 1857 (but curiously not to 1859). Their work follows much the same chronological pattern as Conacher, but it lacks the coherence of development that he saw. The authors describe their methodology as "first making a serious attempt to identify the Peelites and then going on to study their voting records on political, economic, social and religious issues." Their issue-by-issue analysis of Peelite voting records in a series of four- to five-page chapters, interspersed with long lists of names of members who voted with the Peelites, gives the book a feeling of choppiness. They report much useful information, but the conclusions from it remain to be drawn.

Perhaps it is a failing of a sociological rather than narrative age in historiography, but one finishes both these books with an unrequited desire for a deeper explanation of what caused the Peelites to subsist so long as a group. Both works investigate and then reject the hypothesis of a social or economic base for their membership. Conacher does so emphatically after having seemed to move toward a social interpretation of the Peelites in *The Aberdeen Coalition* (pp. 549–50). In a period when English society was in a tense state of "equipoise," it is hard to be satisfied with an explanation of a major shift in political alignment in terms of loyalty to a leader and a set of general policy sentiments reinforced by hostility to the leaders of the major political groups. One asks what were the social motivations for that change. Yet the investigations of Professors Conacher and Jones and Erickson baffle the question and leave us with the perhaps insoluble conundrum of what really made the Peelites.

WILLIAM H. MAEHL, JR.
*University of Oklahoma*

PAUL SMITH, edited with an introduction and notes by. *Lord Salisbury on Politics: A Selection from His Articles in the* Quarterly Review, *1860–1883*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1972. Pp. viii, 387. $21.00.

ANDREW JONES. *The Politics of Reform, 1884*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1972. Pp. xiv, 281. $19.50.

A transition from early stridency to mature sobriety is not uncommon among politicians of the Right as well as the Left, but few can have embodied this metamorphosis more strikingly

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than Lord Salisbury. Prime minister for almost fourteen years at the end of the nineteenth century, successor to Bismarck as the leading statesman of Europe, ponderous of figure and with a billowing white beard, he came to stand in popular estimation for unruffled, tried, and sober judgment, a personage as reassuring as the queen herself. Both in foreign policy and in cementing the alliance between Conservatives and Liberal Unionists he had proved himself master of the arts of conciliation. Yet he made his mark initially through the pages of the *Saturday* and *Quarterly Reviews* as the most abrasive exponent of right-wing Conservatism, uncompromising both in its principles and in its condemnation of the leaders of the Conservative party, particularly Disraeli, who tampered with these principles in pursuit of power.

Seven of Salisbury's *Quarterly Review* articles on domestic politics are now reprinted; and though impressive for their seeming cogency and patent intellectual resourcefulness, they are startling in their unabashed defense of wealth and privilege. They were intended, as the introduction explains, "to sustain, or create, tensions, to exacerbate fears, and to inflame feelings, on the assumption that political calm had a dangerous tendency to corrode the vigilance and energy of the conservative classes, and that the way to get moderates into the conservative ranks was to frighten them there" (p. 106). The approach was less than convincing as a strategy of argument, and politically it was self-defeating. Salisbury's use of a utilitarian, unromantic method of debate lent new attractiveness to familiar Conservative conclusions previously in disrepute as the product of unthinking reverence for the status quo; but this favorable impression was offset by exaggerations into which he was led by his terror of democracy. His insistence upon class as the determining force in British political society would have reduced the Conservative party to powerlessness after 1867 if it had not been for Disraeli's contrary rhetoric about "one nation."

Paul Smith's introduction integrates a description of Salisbury's febrile psychological make-up with the analysis of his polemic and in doing so conveys with brilliance an appreciation of the dynamics of the young politician. Smith is less successful in discerning the process of maturation between the early 1870s and the mid-1880s—years admittedly when Salisbury wrote very little—from which he emerged to become the almost perennial prime minister.

Andrew Jones probes the penultimate phase of this process in his study of the jockeying for political advantage in which the passage of the Third Reform Bill was embedded. His study, of course, does not concentrate on the Conservatives; the Reform Bill was enacted under a Liberal government and was the result of Liberal initiative. Yet it is internal Conservative politics that Jones elucidates best. For, despite the fight for leadership of the Conservative party, the jungle of Liberal politics was more tangled and its denizens less united; if either side won over the bill, it was the Conservatives; and the only fairly clear victory on a related issue was Salisbury's in eliminating Northcote as a rival. Jones reveals Salisbury in a character for which Paul Smith provides no forewarning—as a master of political maneuver.

Jones's book is modeled on Maurice Cowling's study of the passing of the Second Reform Bill. Both are based upon a premise about the nature of political determination, a belief, as Jones words it, in "the importance of political in-fighting, the significance of duplicity as a means of conciliation, and the overriding impact of ambition or frustrated ambition on the wills and actions of politicians" (p. 237). The premise leads both men to turn previously familiar but distorted stories into accounts of sometimes unfathomable complexity. The reader is made aware of a tangle of motives that, the authors argue, explain the course of the story and the nature of its outcome. This approach provides a sophisticated corrective to the Whig history, which saw Reform Acts as responses to popular or Radical demand. However, in reacting against the naive simplicities of Whig narrative, it tends to overcomplicate the skein of private motives. Jones revels in complexity; instead of unraveling the motives of each participant, he maximizes their intricacy. Furthermore, Cowling and Jones fall into a simplicity that is the reverse of that of the Whigs: their vision is restricted to Parliament, with excursions onto the public platform; and the popular dimension of politics, more difficult to estimate because it is less articulate and harder to document, disappears. The event it-

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self and the process of which it formed a part also tend to disappear. Any reader not already familiar with the story will soon find himself lost, or, if not quite lost, will emerge with an unconvincing impression of the process by which Britain became democratic. It cannot have been just a matter of political infighting.

PETER MARSH
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BRIAN L. BLAKELEY. *The Colonial Office, 1868–1892.* (Duke Historical Publications.) Durham: Duke University Press. 1972. Pp. xv, 195. $9.50.

R. J. HIND. *Henry Labouchere and the Empire, 1880–1905.* (University of London Historical Studies, 31.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1972. Pp. xii, 271. $15.25.

These are vital studies for an understanding of how the Colonial Office was reorganized and functioned to deal with the problems of an expanding British Empire and for an understanding of one of the most perceptive critics of British imperial expansion. Both are the results of intensive research, analysis, and interpretation.

Mr. Blakeley's study confirms the fact that the far-reaching administrative changes experienced by the British bureaucracy during the nineteenth century were more the result of the initiative of the political leadership and of the force of circumstance than the design of the bureaucrats. The Colonial Office was no exception. But, hitherto, administrative studies of the Colonial Office have concentrated on Sir James Stephens, who created the "system," and Joseph Chamberlain, who modernized the department, and have ignored the very important changes that occurred between 1869 and 1892. The major reform of the organization and practice of the Colonial Office was the work of Robert G. W. Herbert, Robert Meade, and the earl of Kimberley. If such civil servants as Herbert and Meade accepted reform, it was more the consequence of such outside pressures as the Treasury and the increasing volume of work (resulting from the changing relationship between Britain and her colonies) than from personal inclination.

Since the appointment of Herbert as permanent undersecretary in 1871 marked "a new era of interest" in the colonies and he served longer than any of his predecessors and successors, his work comprises the core of Mr. Blakeley's study. Herbert had served briefly as Gladstone's private secretary, as prime minister of Queensland, and as a civil servant at the board of trade before coming to the Colonial Office. Amiable and pragmatic, Herbert was an excellent administrator but more adept at managing men than developing long-range policies. Like his colleagues in the Colonial Office, Herbert was chary of assuming additional responsibilities for the department. Thus, after the reforms of 1870 and 1872, the Colonial Office was "a satiated establishment, primarily concerned with maintaining the status quo." While the Colonial Office increased in size during the 1890s and became very influential under Chamberlain, the organization and routine of the department remained much the same.

The Colonial Office was often the target of Henry Labouchere's unrelenting campaign against British imperial expansion. Mr. Hind not only explains the rationale of Labouchere's hostility to imperialism, but he makes a valuable contribution to an understanding of Labouchere's very complex personality and does much to correct A. L. Thorold's imperfect biography of the man.

An agnostic with an acutely critical and penetrating mind, Labouchere was in Parliament from 1880 to 1905 and the founder and editor of the very successful weekly, *Truth*. The fact that he used *Truth* to advertise his radical politics gave Labouchere a "privileged position in Parliament." He had no use for chivalry and sentiment in politics and took advantage of the heterogeneous Liberal party and the vexing Irish Question to effect an alliance between his radical following and the Irish nationalists. Basically, Labouchere's creed comprised antipathy to the House of Lords; the advocacy of political and social reform, Irish home rule, and the reduction of armaments; bitter opposition to women's suffrage; and unrelenting criticism of imperialism. He was more a critic of imperial policies than of the Empire and, with the exception of Egypt, consistently opposed British intervention in Africa and Asia. Yet, although he possessed a very shrewd understanding of nationalism, Labouchere was uninterested in the native population of the Empire. His major argument against imperialism was

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that aggressive colonial policies weakened the British Empire by imposing heavy financial and military burdens on the nation. Britain, argued Labouchere, could devote its energies to internal reform if it eschewed imperialism.

Labouchere, however, was a failure because of the distrust engendered by his deviousness and lack of restraint. Indeed, his political career terminated in 1905 as a result of the revelation of his use of the press to manipulate the stock exchange to his advantage. Labouchere was his own worst enemy. His great capacity and fateful prognostications were rendered ineffective by ruthlessness, contempt for authority, and disdain for political and public opinion.

A felicitous style and excellent bibliographies enhance the worth of these books. Messrs. Blakeley and Hind merit praise for jobs well done.

JOSEPH O. BAYLEN
*Georgia State University*

A. B. COOKE and J. R. VINCENT, editors. *Lord Carlingford's Journal: Reflections of a Cabinet Minister, 1885*. New York: Oxford University Press. 1971. Pp. 173. $8.00.

Lord Carlingford's journal covers his last six months as a cabinet minister, during which he attended thirty-seven cabinet meetings. This was almost his only political activity, though he performed his fairly light and politically uncontentious duties as lord president adequately. In these six months the cabinet nearly broke up over the problems of the Egyptian budget, the government majority of only fourteen after the death of Gordon, and the task of drawing up coercive legislation for Ireland; eventually it fell over liquor taxation. The four issues were unconnected (except that the prospect of defeat on the budget looked like a merciful relief from the Irish coercion problem), and the possibility of war with Russia over Afghanistan passed almost unnoticed as a moment of relaxation between the Sudan and the Irish problems. At the level of description Carlingford was undoubtedly talented, but what historians would really like is an explanation of what cabinet ministers thought they were doing. Because Carlingford did not meet his colleagues very often, and few of them valued his support enough to try explaining their approach to him, the journal is rather like a set of minutes: very welcome, but not as informative as something more gossipy would be. On the other hand, Carlingford was more impartial than any of his colleagues, though he clearly leaned to the Whig imperialist side.

The introduction is largely devoted to Gladstone's attempt to induce Carlingford to resign in 1884; this account is properly sympathetic to Carlingford but shows no understanding of the problems of a prime minister trying to reshuffle his cabinet when he was not allowed to dismiss ministers. The notes to the text are helpful but not without faults.

TREVOR LLOYD
*University of Toronto*

W. J. READER. *The Weir Group: A Centenary History*. With a final chapter by the Right Hon. VISCOUNT WEIR. London: Weidenfeld and Nicolson. 1971. Pp. x, 238. £3.00.

"If ever there was a time and place to set up in any business connected with marine steam engineering, the time was the early eighties and the place was the Clyde." In this environment James Weir, a former apprentice in the river's leading yards, patented several inventions for pumps and auxiliary machinery and within a decade became a world leader in a highly specialized field of manufacturing. Growth was ensured by the triumph of steam over sail, the naval race between the powers, and munitions production during the First World War. After 1919 the bottom dropped out of the firm's markets and the scale of its operations had to be drastically reduced. Attempts at diversification yielded no commercial benefit except in the case of rotary pumps. "We do not believe our brains are good enough," Lord Weir wrote, "to deal efficiently with more than one industry." Not until after the Second World War was the character of the business transformed. A public company was created in 1946, nine years later a holding company, and then in 1967 the Weir Group was formed with five manufacturing divisions comprising forty-five different companies. Within a generation, as Viscount Weir shows in the final chapter, the firm became an international multiproduct enterprise supplying steel castings, factory-made buildings, multiflash distillation plants, and aircraft equipment.

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Unfortunately, less than a third of the narrative is devoted to the remarkable development of the company since 1945. Mr. Reader's main emphasis is on the fortunes of a family firm in Scotland, and a great deal of attention is devoted to its generally unsuccessful attempts to diversify. The author's conclusions therefore tend to be ambiguous: "the story . . . shows contradictory and co-existent inclinations towards caution and adventure, almost amounting to a split mind, which was characteristic of Weirs' second generation management." Furthermore, Reader's judgments on the firm's labor force are uncritical and inconsistent. Whatever the employers' reactions may have been, the author's readiness to blame Clydeside workers for the firm's difficulties during and after the First World War is hardly justified by the evidence. The picture he paints of "controlled Ludditism in engineering" has to be seen in the context of undistributed profits worth a million pounds being allocated to a private company's handful of shareholders. In short, this account, though readable, is one-sided and superficial. Consequently Reader's broader generalizations, which are limited to comparisons between Weir's performance and the contemporary malaise of British industry, are less valuable than they perhaps might otherwise have been.

GORDON RIMMER
*University of New South Wales*

RODNEY BARKER. *Education and Politics, 1900–1951: A Study of the Labour Party.* New York: Oxford University Press. 1972. Pp. viii, 173. $10.25.

Education was, during the first half of the twentieth century, a matter of continuous if secondary concern for the British labor movement. By his systematic examination of the Labour party's policy in this area over five decades Rodney Barker attempts to provide a basis for generalizations concerning the party's approach to noneconomic issues and thus to the nature of British socialism. He finds a continuity in Labour's educational policy between the years preceding and following its adoption of socialism in 1918, with the party promoting, in both instances, a program indistinguishable from that of progressive Liberals. The raising of the school-leaving age and fairer access to the existing educational system were the goals throughout most of this period. It was only in the years following World War II that schooling began to be regarded not simply as a benefit that should be more equitably shared by all classes but as a weapon for attack on the class structure itself.

Barker's findings therefore reinforce the generally accepted view of the Labour party in this period as sober, respectful of existing institutions, moderate in its goals, and pragmatic in its approach to reform. Although the author's judgments are generally cautious he perhaps goes too far in concluding from Labour's lack of a distinctive policy in education that the party's adoption of socialism in 1918 was "largely an illusion" (p. 35). As he himself observes, the socialism of the day was focused on economic rather than social reorganization, and its lack of impact on the party's thinking on education gives little indication of its influence in areas of more central concern.

Those unfamiliar with the Labour party's record will find in this study a lucid treatment of one aspect of its domestic policy. By utilizing the papers of the party's advisory committee on education as well as those of the committee's most influential member, R. H. Tawney, and of Charles Trevelyan, president of the Board of Education in the two interwar Labour governments, the author has provided an account of the internal debate surrounding educational questions, which will be illuminating as well to students of the labor movement.

CATHERINE ANN CLINE
*Catholic University of America*

C. J. LOWE and M. L. DOCKRILL. *The Mirage of Power.* Volume 1, *British Foreign Policy, 1902–14*; volume 2, *British Foreign Policy, 1914–22*; volume 3, *The Documents: British Foreign Policy, 1902–22.* (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1972. Pp. xii, 168, xiii–xxiv; xiii, 169–422, xv–xxvi; xvii, 423–759. $10.00; $10.95; $13.25; $31.75 the set.

Lowe and Dockrill's work, based on specialist studies and their own research, is valuable for the insight it gives into the pressures on makers of policy from leading politicians and, after 1905, permanent officials at the Foreign Office. A more sympathetic view than usual is taken of the principal actor, Sir Edward Grey. Atten-

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tion is seen increasingly concentrated on Europe, where Grey was convinced the potential troublemaker was Germany. Rejecting alike thoughts of isolation or alliances, Grey's unchanging solution was to cultivate friendship—at a price—with France and Russia. Despite sporadic criticism from colleagues after 1907, steps toward a limited rapprochement with Germany were only taken from 1912 onwards.

Grey's handling of successive crises brings few adverse comments, not even his self-confessed "mistake" of sanctioning secret military conversations with the French (leaving Asquith himself almost certainly in the dark until 1911). There are a few important lacunae. To Lansdowne's 1902 agreements with Japan and the United States, aimed at reducing worldwide commitments, should certainly be added that with Italy—researched in depth by Enrico Serra and others who declared it a vital factor in the making of the *Entente Cordiale*. There should surely be some mention also of the policy enshrined in the unratified Declaration of London (1909), which left Grey in 1914 morally committed to rules of warfare at sea seriously circumscribing British sea power.

There are well-researched chapters dealing with wartime attitudes toward Italy, the Balkans, and peace aims, as well as an illuminating discussion on Far Eastern and Russian problems. Yet voluminous Foreign Office records concerning blockade and neutral rights—which kept the Committee of Imperial Defense historian A. C. Bell busy until 1937—have hardly been sampled, let alone analyzed. An unwise policy toward neutrals, too harsh or too yielding, would inevitably have radically affected the outcome of the war.

When the element of continuity provided by Grey disappeared, several cabinet members (and public figures) acted as if foreign policy was too important to be left to the Foreign Office. Churchill carried the day for intervention against the Bolsheviks. Lloyd George himself dominated decision making between 1919 and 1922. When uncoordinated policies and disagreements over the Middle East and Germany ended collaboration with France, the simultaneous evaporation of Britain's wartime power meant that Lloyd George's vision of the future lacked the backing that power confers.

The volume of documents is uneven in its support of the text. Indeed, there are no documents to illustrate the making of the entente with France or prewar relations with the USA, and, pro rata, the period after 1914 has five times the documentation of that before. It does seem a pity also that the authors have subscribed to the present tendency not to use foreign documentary sources, with the exception of one important volume of *I Documenti Diplomatici Italiani*. Statesmen, after all, discuss policy and negotiate with foreign diplomats. However, in general, this book is to be commended as a reliable guide to the aspects of foreign policy discussed, though students will not be able to rely solely on the volume of documents, which avoids official correspondence printed elsewhere.

ARTHUR MARSDEN
*University of Dundee*

IAN H. NISH. *Alliance in Decline: A Study in Anglo-Japanese Relations, 1908–23*. (University of London Historical Studies 33.) [London:] University of London, Athlone Press. 1972. Pp. xii, 424. £7.50.

WM. ROGER LOUIS. *British Strategy in the Far East, 1919–1939*. New York: Oxford University Press. 1971. Pp. 284. $10.50.

The two authors have covered Anglo-Japanese relations from 1894 to World War II. In 1966 Nish published *The Anglo-Japanese Alliance: The Diplomacy of Two Island Empires, 1894–1907* (*AHR*, 72 [1966–67]: 530–31). In well-researched detail it presented the converging interests of Britain, an overstrained world power, and Japan, modernizing and beginning to act in the game of power politics on the Asian scene. The diplomatic and strategic answer to the Anglo-Japanese fear of Russia was the Alliance of 1902, which lost some of its urgency after Japan's defeat of Russia in 1905. *Alliance in Decline* continues the story painstakingly, perhaps too painstakingly, for material is repeated not at all carelessly. For it Nish again has used a wide array of printed material in English, Russian, and Japanese as well as archival and documentary collections in Japan, Britain, the United States, and Australia. His scope extends from the renewal of the alliance in 1911, through the World War years of Japa-

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nese expansion in China and of Japanese naval aid to Britain, to the calculations about Britain's resources for the future and her new needs and associations that led to the replacement of the alliance by the Four-Power Pact.

Louis covers some five years of the same subject in presenting the story from 1919 to 1939, based mainly on the printed Foreign Office and archival British cabinet and Foreign Office documents. "Strategy" in the title is meant to describe an area broader than official policy, which by 1921 had to be reformulated for the Far East. The points of decision are fairly familiar: China was alternately weak or in convulsion, and Japan, nearby and strong, rushed into the vacuum; Japan's Chinese governance was exclusionist and imperialist in any sense; the United States was even more hostile to Japan in China than Britain was, though British investments in China greatly outran American capital and the British lion properly had a leonine share of trade and services in China; American hostility to the Anglo-Japanese Alliance stimulated proposals for American naval construction, and the prospective naval race was too costly for the British government, apart from the near-certainty that it would endanger future association with the United States, proclaimed by many Britons as an essential policy; the same American hostility complicated Britain's relations with the British Dominions; and there were pervasive racist fears and suspicions, as Louis well reveals. Britain abandoned the Japanese alliance even less readily than she had abandoned her mistrust of entanglements. The subject made its way through cabinet discussions, policy memoranda, an imperial conference, and finally the Washington Conference, where, Nish records, a Japanese comment to a British observer was: "At any rate you gave the Alliance a splendid funeral" (p. 383). Curzon did not favor a prospect in which the hand of an allied Britain, even with the minimal force it had in the Far East, would not be available to restrain the possible recklessness of Japan. In the thirties George Sansom, a historian and diplomat, countered thoughts that Japan was changing wholly for the worse: against them he thought that things would not have turned so ill if the alliance had been maintained. More recently Malcolm Kennedy has expressed his misgivings about the estrangement of Japan and Britain.

Nish has written a rounded and complete work. Inevitably, Chinese documents are not available to him and Chinese reactions are seen through the eyes of foreigners, though happily a number of foreigners. Rarely, but it does happen, he inaccurately describes his references to some people treated in the first volume as "characterizations," which they are not. This is but a slight fault when placed against the merits of his thorough, thoughtful, and model work.

Louis with a larger subject and a longer time period is somewhat less equipped with sources than Nish. His book, nevertheless, appears to have been a decade in maturing and comes from James Crowley and Gaddis Smith as Louis's "contribution to the Far Eastern International History Seminar at Yale University" (p. 16). *British Strategy* has the judiciousness that may be expected from "writing in committee" (p. 16). It is as well unexpectedly lively and provocative.

M. A. FITZSIMONS
*University of Notre Dame*

LAWRENCE F. BARMANN. *Baron Friedrich von Hügel and the Modernist Crisis in England.* New York: Cambridge University Press. 1972. Pp. xiii, 278. $18.50.

This clearly written book meets the need for a detailed and accurate account of the role played by Friedrich von Hügel, the religious philosopher and Biblical critic, in the Roman Catholic Modernist movement over sixty years ago. Based on extensive research in relevant archives and manuscript collections in England, it should end the confusion that has often surrounded von Hügel's role. While it emphasizes his involvement in the movement in his English environment, it also deals with his international activities in behalf of the Modernist cause. Professor Barmann has made a useful contribution to the history of Catholic Modernism.

Despite Barmann's admirable research, his study has a few substantial weaknesses. First, Modernism is defined too broadly as "the more or less simultaneous awakening of a number of Roman Catholics, . . . all strikingly aware of the intellectual and other advances of their

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own era and milieu, to the serious inadequacies of the Roman Catholic ecclesiastical system of thought and practice then current" (p. x). This "awakening and awareness," however, also characterized a number of Catholics who were not Modernists. Intellectual ferment and attempts at renewal were more widespread in the Roman Church in the late nineteenth and early twentieth centuries than Barmann's book indicates. Second, the book does not give an impartial account of the Modernist crisis. Barmann is entitled to his interpretation portraying von Hügel's views of issues as generally right, but Barmann's study depicts several opposed views inadequately. There was more substance in, for example, the positions of the moderate liberal Catholic Wilfrid Ward and the philosopher Maurice Blondel, who at times disagreed with von Hügel, than this study indicates. Still less adequately represented are the ecclesiastical authorities' views of the issues in the Modernist crisis. Last, the theological and philosophical issues in the crisis are not dealt with in sufficient depth. Thus the anti-immanentism and antisubjectivism that developed in von Hügel's thought and that distinguished him from some of the other Modernists receive only passing mention, although he attached great importance to these issues, which were, indeed, central ones in the crisis.

On balance, however, the fresh information resulting from Professor Barmann's diligent research and the real contribution that the book makes by describing von Hügel's involvement in the Modernist movement more than offset the drawbacks.

WILLIAM J. SCHOENL
*Michigan State University*

PETER ROWLAND. *The Last Liberal Governments: Unfinished Business, 1911–1914*. New York: St. Martin's Press. 1971. Pp. xiv, 405. $12.95.

Restricting himself severely to the political events from 1911 to 1914, Peter Rowland has completed his useful, if somewhat tedious, summary of the Liberal governments' activities in the decade before the outbreak of World War I. Like its predecessor *The Promised Land, 1905–1910* (*AHR*, 75 [1969–70]: 499–500), the present work plods its way from cabinet to cabinet and from Parliamentary session to Parliamentary session meticulously describing who said what to whom and where, but seldom, except in a brief chapter of conclusions, speculating very seriously about the social environment within which its Liberals made the cautious and pragmatic moves that added up somehow to what came to be identified as their "policies." Yet *Unfinished Business* is a substantial improvement over its predecessor.

By and large, the thesis of the first volume, that the last Liberal governments looked back to the world of the nineteenth century rather than forward to that of the twentieth, is reiterated in this second study. Once again Rowland gives us little insight into why that should have been, although he does take issue with George Dangerfield and Elie Halévy, to say nothing of Stephen Koss, in denying that the decline of the Liberal party was both natural and inevitable. Without the intervention of war, Rowland speculates, the Liberal party might have been reinvigorated, the Labour party might not have emerged as a viable substitute, all sorts of possibilities might have developed. But speculation aside, Rowland is in fact on very sound ground in pointing out the irony of the contradiction between what the Liberals proposed and what they are most remembered for. Successive Liberal governments, he points out, had not come into office intending to create anything like a "welfare state." The social reform measures they passed were regarded by the old guard as the "tiresome price that had to be paid for having Lloyd George in the Government." Yet it is social reform for which the Liberals of 1905 to 1914 are given high marks, while the "real" Liberal program—educational reform, licensing reform, abolition of plural voting, reform of the House of Lords—failed to reach the statute book and is almost forgotten.

In another way the author brings a large element of balance to the rather dramatic representations of a George Dangerfield or even an Elie Halévy. Despite labor unrest, suffragette disruption, and Ulsterite mobilization, British society was not really falling apart at the seams by 1914. Rowland makes clear, I think, that each of the three threats was considerably less extreme than as described in some overheated accounts. The early twentieth-century "age of violence" appears to pale when subjected to detailed scrutiny.

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Although *Unfinished Business* shares some of the weaknesses of *The Promised Land*—and one may add that too much of too little relevance appears in the footnotes or that it is unnecessarily thin on foreign policy—it is on the whole a more persuasive account. The last Liberal government comes through as a group of politicians, none of them—not even Lloyd George or Churchill when they concerned themselves with reform legislation—really understanding the social changes that were causing the world they knew to disappear, but most of them trying to do their best within their narrow limits of perception. It is on the whole a more persuasive picture than we have had in the past. One minor point: Lloyd George's mistress was of course Frances Stevenson, not Frances Stevens.

HENRY R. WINKLER
*Rutgers University, New Brunswick*

J. M. WINTER and D. M. JOSLIN, edited and with an introduction by. *R. H. Tawney's Commonplace Book.* (Economic History Review Supplement 5.) New York: Cambridge University Press. 1972. Pp. xxiv, 88. $6.50.

The publication of his *Commonplace Book* by the *Economic History Review* is clearly intended as a memorial to R. H. Tawney, the most respected, even venerated, of the generation of "committed" historians, which included the Hammonds and the Webbs. (The volume also includes a tribute by M. M. Postan to the Cambridge economic historian David Joslin who, with J. M. Winter, wrote the introduction to this work before his untimely death in 1970.) As this book makes plain, Tawney quite early defined the views that were to characterize his brand of socialism and planned a scholarly *oeuvre* that would bear witness to his faith in a morally just, genuinely Christian social order.

The *Commonplace Book* was begun in 1912 when Tawney was thirty-two and was continued for two years. After Balliol and service at Toynbee Hall, Tawney had just completed his *Agrarian Problem in the Sixteenth Century* (1912) and was a Workers' Educational Association lecturer at Manchester. In this intellectual diary he set down his faith in an absolute moral standard, in equality, in "the supreme value of every human personality," in a society in which "there is no rivalry but only service." He saw capitalism as a thoroughly immoral order in which men were not seen as personalities but used as tools, and he attacked defenders of privilege like Alfred Marshall, whose eulogies of "economic chivalries" Tawney found repellent. Nor did he spare socialist utilitarians like the Fabians who sought efficiency and material improvement rather than an egalitarian society.

Tawney's denunciation of material progress achieved at the expense of a just and consequently satisfying social system, his contempt for the spirit of "getting-on," and his wish that men come to despise wealth and return to the ideals of "voluntary poverty and renunciation" have both a medieval and a contemporary New Left flavor. Moreover, far removed from the stereotype of a "moderate" Christian socialism, he welcomed violent revolution with its "risk of facing a generation of disorder" that he believed necessary to realize the great moral change he envisioned, making in the course of his argument some highly perceptive suggestions about the revolutionary process. Though it is somewhat disconcerting to find this link to Cromwellian fanaticism in Tawney's Christian egalitarianism, the volume remains a further testimony to his passion for social justice that made him an inspiration to historians and a much-loved saint of socialism.

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KENNETH YOUNG. *Sir Alec Douglas-Home.* Teaneck, N.J.: Fairleigh Dickinson University Press. 1971. Pp. xii, 282. $15.00.

As is generally known, this is a time of financial difficulty for many of America's university presses. When, therefore, a biography of this kind is published by a university press that cannot be numbered among the more opulent, it is not altogether unreasonable to ask: Is this book really necessary? Given the fact that an English edition already existed, the question is is not entirely academic. American university presses, operating on very limited budgets, ought not to be using their funds for publications like this.

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Is the book a very bad one? Not at all; it resembles dozens of others published in this country and in the United Kingdom on major contemporary political figures. It is a good journalistic account of Sir Alec Douglas-Home's life. It is the kind of book that makes an admirable Christmas gift. However it cannot be said to have any major importance for a scholarly audience. It is clear that Kenneth Young knows Sir Alec well, likes him, and has been granted a good number of interviews. In theory, a very personal account might have resulted. It is regrettable that such an account is not forthcoming. The pages on Sir Alec's childhood, youth, and early adulthood, while unobjectionable, tell little that is at all revealing. More serious, perhaps, is the failure to provide new evidence on public events, not only during the brief period that Sir Alec served as prime minister, but also during the time when he served under Eden and Macmillan in various cabinet posts, including that of secretary of state for Commonwealth Relations, leader of the House of Lords, and foreign secretary.

The quality of Young's account is perhaps best expressed by indicating the information he provides on various moments of crisis during recent years. Thus, for example, on Suez we learn that Sir Alec favored Eden's policy, though he himself would have preferred that the British take military action alone, independently of the French. Eden insisted on the Commonwealth not being informed in advance of the military operation, and Home agreed with this decision. Sir Alec was later severely criticized for this, but there is nothing to suggest that he thinks it was a mistake. He believes that the military operations were not handled well—an understatement—but otherwise sees nothing terribly wrong with the Suez undertaking. Sir Alec's biographer tells us that the thing that did "rankle" Sir Alec was "the American betrayal." This is the quality of the reportage on what was a major turning point for Britain in the 1950s.

The character of the analysis may be rendered also by reflecting on what Young has to say about the United Kingdom's relations with the United States during the Kennedy era, when Sir Alec served as foreign secretary. At the time of the Cuban missile crisis we are told that Kennedy and Macmillan were on the "hot line" nightly and that "using the 'hot line' is not easy for the unmechanical." We are also told that Sir Alec believed that "Britain's counsel had a considerable influence." That is about all the information we get. As for the Skybolt affair and the meeting at Nassau, no one reading the author's two pages on the issue could have any idea of why it was important. Even the conjecture of the effects it had on de Gaulle is very cursory.

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EDWARD NORMAN. *A History of Modern Ireland.* Coral Gables, Fla.: University of Miami Press. 1971. Pp. 330. $7.95.

PATRICK O'FARRELL. *Ireland's English Question: Anglo-Irish Relations, 1534–1970.* New York: Schocken Books. 1971. Pp. 336. $11.50.

In one of her theater chronicles, Mary McCarthy wondered why certain actors were on the stage. She asked not why a producer had been so misguided as to employ these untalented players but why the players themselves were so deluded as to believe they could act. These books raise questions of that kind: why they were published is not nearly so baffling as why the Reverend Dr. Norman and Professor O'Farrell wrote them at all. Although the books have different "messages," they are alike in that what is true in them is not new, and what is new is not true. The reader can only be amazed at the authors' laying claim to the serious attention of their colleagues with works so ill-considered as these.

Norman contends that the Irish Revolution (1916–23) was neither a good nor a necessary thing and was certainly not the product of oppression—a view that is hardly original, having been argued with great skill by Nicholas Mansergh and Oliver MacDonagh. What is original here is Norman's contempt for the Irish and for their resistance to the process of achieving "the very real assimilation of Irish life to the general British type," for "the most advanced society in the world was attempting the assimilation of a less developed one." Those who fought against becoming West Britons were "small coteries of litterateurs," "racists" like Daniel Moran, and various *lumpen* types.

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Many of them seem to have been emotionally unstable, if not sexually perverse. Norman seems not to know that the Irish Revolution is not the only one that was "made" by those without an established place in society.

Norman's gifts for trivializing Irish history are so great that his book does not deserve to be taken seriously. He seems to believe that he has bestowed himself on material that is beneath him. His book may, however, have a certain interest as a document in English social history: it can be read as the act of someone apparently not born in the purple aping the attitudes (or the supposed attitudes) of the aristocracy. Norman quotes Archbishop Manning to Lord Grey: "I do not think that Englishmen are enough aware of the harm some among us do by a contemptuous, satirical, disrespectful, defiant language in speaking of Ireland and the Irish people." It is hard to imagine Norman's being easily excelled in the language that Manning was complaining about. His disdain for the Irish varies according to their social class. Anglo-Irish gentlemen like Parnell and Thomas Davis are treated less derisively than the Irish-Irish. Norman is a master of innuendo as the following example selected at random shows: "The district [Irishtown, County Mayo] had just been disturbed when a new owner had evicted his tenants from the largest local estate. He was a Catholic priest called Canon Burke. In the same month Parnell joined in with one of his militant utterances at Limerick: 'Stand to your guns, and there is no power on earth which can prevail against the hundreds of thousands of tenant farmers of this country.' An unearthly power, indeed, visited the West during the summer. At the height of the agrarian disturbance in Co. Mayo, in August, an apparition of the Virgin appeared beside a Catholic chapel in the small village of Knock." Mean-spiritedness sustained for more than three hundred pages ought to get some kind of prize.

O'Farrell's message is the reverse of Norman's: The Irish Revolution was the natural and inevitable outcome of Irish history, for the Irish Question was not political but "historical and religious—a question of identity." The tired old sentiment, "To be Irish is to be Catholic," has been dressed up in the new clothes of "identity." O'Farrell's is a genuinely obsessive performance. He does not argue from evidence; he repeats his propositions, and he defines historical realities into and out of existence and then deduces from his curious definitions. It is not enough for O'Farrell to declare that the essence of Irish nationalism was religion, he must deduce from that definition that a revolutionary solution was therefore inevitable, for such constitutional movements as repeal or home rule were "British" (as opposed to Irish and Catholic) solutions. The Irish people wanted, from the Reformation on, apparently, complete independence from England. How O'Farrell knows what the Irish people (whoever they are) wanted at any given time is not clear.

Nor is O'Farrell content with deducing the inevitability of revolution from the religious identity (even though the opposition of the Catholic Church to the revolutionary tradition is one of the facts of Irish history). He must identify Catholicism with spirituality. The conflict between Ireland and England was the conflict between the spiritual, otherworldly Irish and the secular, materialistic British. A Catholic bishop mulcting the faithful to build a cathedral was apparently engaging in a spiritual act, and the Evangelical revival in England apparently had nothing to do with religion (or spirituality). Nor, since the members of the Irish Catholic middle class were materialistic, will O'Farrell permit them to be called truly Irish. The land hunger of the peasants does not mean that they were materialistic (and un-Irish) because the landlords were, after all, Protestants—including, it would seem, priests like Canon Burke. The reader follows the argument with a kind of fascination, watching O'Farrell being hoist on his own petard.

It is not, of course, O'Farrell's deductions alone that are questionable. So are his premises. Does he seriously mean to suggest that had the Irish been conquered by a Catholic power, there would have been no resistance, no Irish nationalism? If so, he knows very little about nationalism elsewhere. Both of these writers seem to know just enough history to confirm what can only be called their prejudices.

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DONALD HARMAN AKENSON. *The Church of Ireland: Ecclesiastical Reform and Revolution, 1800–1885*. New Haven: Yale University Press. 1971. Pp. xiii, 413. $15.00.

"The Church of Ireland was not simply a minority church, it was one surrounded by a sea of hatred. . . . Representing a narrow constituency, surrounded by a hostile majority, controlling great endowments, uncertain of its priorities, inefficient in its administration, the Church of Ireland was ripe for either reform or revolution. In the nineteenth century it experienced both." To delineate these changes in the established Anglican Church in Ireland, Donald Akenson of Queen's University, Kingston, Ontario, has purposely focused on the Church as an administrative institution whose "primary purpose . . . was the delivery of pastoral services to the Irish Anglican community." Disclaiming any concern with—and one suspects, any interest in—theological, political, or social intricacies, he delights in rigorous analysis, statistical tables, and the minutiae of administrative change.

Within these deliberately narrow limits Professor Akenson has written a competent, completely reliable, detailed monograph for anyone interested in the history of institutions. A thorough description of the administrative system of the Church in the eighteenth century reveals it as an exaggerated example of all the evils of establishment. Secure, indolent, and Erastian, its bishops were selected by a grossly political process, and there was no means of insuring residence of either bishops or parish clergy. During the first thirty years of the nineteenth century, an era of "graceful reform," much in this picture changed. Obvious improvements in efficiency—increased clerical residency, numbers of benefices and churches, and assiduousness of the parish clergy—enabled the Church to fulfill its primary purpose as defined by Professor Akenson.

As for the further, more drastic reforms of the 1830s to 1860s, Akenson refuses to view them as part of the larger wave of parliamentary reforms (electoral reforms, the poor laws, and so forth) in England. Instead they were the logical outcome of the tithe reform measures of the 1820s, when the principle of parliamentary interference with ecclesiastical property was established. And most important, by having to fight for Catholic emancipation until 1829, the Roman Catholics had become politicized, achieving "a political identity and momentum" that inevitably turned on the Church of Ireland. In contrast to the earlier reforms, which augmented the Church as established, those stemming from the Church Temporalities Act of 1833 were negative, reducing the Church's resources and suppressing a number of bishoprics, and became the prelude for disestablishment. Ultimately, in the face of ever-growing Roman Catholic power, the issue was not increased efficiency, but whether the Church of Ireland should exist as a state church at all.

Gladstone's great Disestablishment Bill is treated in detail with admiration. Akenson is understandably impressed by the way in which the Church weathered the years of reorganization, overcame severe internal friction, both doctrinal and organizational, and also exhibited great financial skills. He uses the term "revolutionary" to describe the emergence of lay influence in Church government and seems surprised that disestablishment resulted in the Church showing all the earmarks of a voluntary church. But considering the economic and social stature of its laity, this might be expected. To me, the interesting fact is that the sudden removal of the state shows how all-important its undergirding support had been for the continuance of an aristocratic, hierarchic structure and how quickly that structure conformed itself to the financial and numerical realities of its members.

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RICHARD COBB. *Reactions to the French Revolution*. New York: Oxford University Press. 1972. Pp. 310. $13.75.

For many years in the history of the French Revolution, as of other subjects, there has been a trend away from the study of decision-making elites toward that of obscure and inarticulate members of society. Lefebvre launched it with his great work on the peasants; Mathiez was thought to be shortsighted in concentrating on such eminent figures as Robespierre; Soboul's work on the sans-culottes and Rudé's on *The Crowd in the French Revolution* are the best-known examples of the new school. Mr.

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Cobb in his earlier writings has carried the same trend forward, but it is doubtful whether his colleagues in it will much welcome his new contribution.

"The purpose of the work," to quote the dignified Oxford jacket, or blurb, "is to indicate that individuals are more important than groups." This commendable generality has a particular meaning for the author. He is at pains to stress the virtues of individuality, of the survival of private life even under conditions of catastrophic public disturbance. He describes for us the experiences and the life-styles of those who, more deliberately, opted out of too handicapped, too ignorant, or too indifferent to know what was happening and likewise of those who, more deliberately, opted out of the Revolution, rejecting any form of social order, actual or proposed, republican or monarchical. The "groups" that he dismisses as unreal, or at least as useless categories in historical explanation, are not only the famous revolutionary bourgeoisie but Soboul's sans-culottes and Rudé's crowd, which are seen as figments of the imagination. The "individuals" are persons on whom information is available in police and judicial records. The book is in effect a sequel to Cobb's *Police and the People* (see my review, *AHR*, 76 [1971]: 1557). But the vein is wearing thin, or becoming more exotic.

We have here the "reactions to the French Revolution" of habitual criminals, murderers, thugs, highway robbers, cheats, prostitutes, and female ogres, topped by a concluding chapter on an especially unpleasant gang of men and women called *la bande d'Orgères*. To this Wild West show of the French Revolution is added the more affecting spectacle of how events were perceived by abandoned children, madmen, and deaf-mutes, with regrets that evidence is lacking for homosexuals. The author detests both Robespierre and Napoleon, and his work will reinforce the idea of the years of Thermidor and the Directory as a time of intolerable disorder, "brief oases of extreme individualism and ungovernment, a trough of anarchy, between the rigid, terrifying systems of the year II and the Empire" (p. 129).

It is unlikely that this highly selected cast of characters, this *reductio ad absurdum* of the historiography of the inarticulate, will really serve the purpose or drive from the stage either the revolutionary bourgeoisie, or the sans-culottes, or the crowd, or Robespierre and Napoleon. It is clear that the researches have been enjoyable to Mr. Cobb and to his students at Oxford, whom he generously cites, but it is not clear what such researches have to teach. The author never asks himself whether a similar exploration of similar records might not yield similar results for any society in which such records were so abundantly kept. The question is how relevant to the French Revolution the book is at all. Much of the violence described was perpetrated against former terrorists, but by no means all; and we are repeatedly reminded that counterterror and counterrevolution were not the same. The author is aware of the criticisms that have been made of him, defends himself from "the trap of anecdotal history," and insists on the humane value of our understanding the plight of the miserable, the disreputable, and the outcast. He confirms what Soboul and Rudé have always said, that the true popular revolutionaries did not come from the lowest and most hopeless social levels. But one concludes by wondering what has become of the study of the French Revolution at Oxford since the days of J. M. Thompson.

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ROGER DUPUY. *La Garde Nationale et les débuts de la révolution en Ille-et-Vilaine (1789–mars 1793)*. (Université de Haute-Bretagne, Institut Armoricain de Recherches Historiques de Rennes.) Paris: Librairie C. Klincksieck. 1972. Pp. 284, 1 map.

We begin to get answers to a question that has absorbed many an hour of earnest, if fruitless, conversation: how much of the difference between French and American historical scholarship is due to the contrast in the organization (as opposed to the quality) of the two educational systems? It turns out that given similar opportunities the two systems produce similar products. Roger Dupuy's *thèse de troisième cycle*, my case in point, is the rough academic equivalent of a Ph.D. thesis. It greatly resembles an American thesis—except that it has been printed, unretouched, for all to see. Its task is to trace the organization and action of the various militias that sprang up to do the

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work of the Revolution in the Breton department of Ille-et-Vilaine from 1789 to 1793.

Dupuy goes at his subject systematically. The gross organization of the book takes us chronologically from the militias of the Old Regime to the National Guard of 1793. The detailed organization follows the varieties of National Guards and of their actions: the urban units, the rural units, the peace-keeping activities of the big-city units, and so forth. The unifying argument, to the extent that there is one, contends that the vigor of the local Guard faithfully represented the intensity of revolutionary sentiment. Dupuy piles up plenty of evidence to that effect, especially in the form of maps depicting the distribution of Guard units at different points in time, the acceptance and rejection of the Civil Constitution of the clergy, military enlistments in 1791, and a number of other signs of revolutionary sympathy, apathy, or antipathy. He also inflates some balloons for the sole purpose of popping them. The most notable case is his attribution (pp. 108–09) to Paul Bois of the view that the militias were insignificant in the countryside, countered by Dupuy's observation (p. 114) that eight per cent of all parishes (half of them cities or big towns) in what would become Ille-et-Vilaine had militias in the latter half of 1789. For some reason Dupuy also takes the considerable agitation of the Breton countryside—resistance to taxation, food riots, attacks on chateaux—as evidence of peasant hostility to the Old Regime and support for the new. It is true, and important, that the militias themselves sometimes led those actions. But it is equally true, and equally important, that the largest part of the effort of militias and National Guard units went into keeping a recalcitrant countryside from blocking the work of a revolution manned chiefly by the men of the cities.

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FRANCISCO MARTÍ GILABERT. *El motín de Aranjuez.* (Colección histórica de la Universidad de Navarra, Number 24.) Pamplona: Ediciones Universidad de Navarra, Consejo Superior de Investigaciones Científicas. 1972. Pp. 477.

ANGEL MARTÍNEZ DE VELASCO. *La Formación de la Junta Central.* (Colección histórica de la Universidad de Navarra, Number 25.) Pamplona: Ediciones Universidad de Navarra, Consejo Superior de Investigaciones Científicas. 1972. Pp. 223.

Although Spanish academic historians have hardly looked at their nineteenth century until recent years, already an active debate has emerged over its basic features. On one side are some members of the faculties of Madrid and Barcelona who stress the innovative legacy of the period; on the other, the school of history of the University of Navarre, run by the Catholic secular institute Opus Dei. Headed by Father Federico Suárez, the latter believes that the dominant forces of the century were essentially traditional. One of its objectives has been to restore the reputation of Ferdinand VII, whom most historians have found vacillating, vengeful, and incompetent.

The two books under review, the most recent of the Historical Collection of Navarre, illustrate the latter approach. Both deal with the year 1808, which saw Ferdinand become king, Napoleon trap him into abdicating in favor of Joseph Bonaparte, and the Spanish provinces rise against the French army. The central topic of Martí Gilabert's long and diffuse book is the *motín* or riot at Aranjuez in March, which caused Charles IV to abdicate in favor of his son Ferdinand. Martí's main point is that the objective of the persons who inspired the riot, and of the common people in the crowds, was only to remove the hated royal favorite Manuel Godoy, not to dethrone the old king. He would thus cleanse Ferdinand's accession of any possible taint. His conclusion appears literally correct, for he finds no personal attacks on Charles, but he plays down the effect on the demoralized king of the breakdown of order and the adulation of Ferdinand by the mobs and wide sectors of Spanish society. Martí collects all known accounts of the events and confronts them for reliability, but he ignores recent theoretical writing, of George Rudé or even Georges Lefebvre, on the motivation of crowds in this period.

Martínez de Velasco's doctoral dissertation on the formation of the Central Junta in September is a much better piece of scholarship, based on extensive original archival research. It is the best available account of the steps taken by the local juntas that arose out of the

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revolt to form a central government independent of the royal councils which had bowed to Joseph Bonaparte. Running through the book is a debate with Miguel Artola of Madrid (whose *La España de Fernando VII* I recently reviewed [*AHR*, 75 (1969–70): 1468–69]). Artola believes that the anti-French rising destroyed the old regime in Spain. Martínez de Velasco argues that the provincial and central juntas aimed at maintaining it. Both views are partly right and partly wrong. Martínez demonstrates that the juntas were made up largely of leading local officials and clergy who feared French innovations (not a new discovery), but he is misleading when he says they wished to preserve tradition. Which tradition? Possibly an idealized myth of the past, certainly not the discredited regime of Charles IV or even the reforming spirit of Charles III. Willy-nilly the situation demanded revolutionary measures, as Artola says. Martínez states at the end that the formation of the Central Junta very possibly was a conscious response to Ferdinand's secret directive from captivity ordering any free Spanish administrative unit to set up a government opposed to the French (and was therefore not a revolutionary act). This conclusion is astonishing, coming after his step-by-step description of the initiatives of the local juntas and his admission that there is no evidence that they or their deputies to the Central Junta knew of the directive until after the Central Junta was formed. It reads almost as if after the dissertation were written he had been encouraged to demonstrate his agreement with an established view, and did so hoping that his readers would not notice the incongruity.

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MICHAEL GLOVER. *Legacy of Glory: The Bonaparte Kingdom of Spain, 1808–1813*. New York: Charles Scribner's Sons. 1971. Pp. xii, 353. $10.00.

Mr. Glover has come nowhere close to writing a history of the Bonaparte kingdom of Spain. Thus my first draft of this review began: "I come here to bury Glover." But that was too harsh. The book should be judged for what it is—a brief history of the Peninsular War. It focuses on Wellington and his opponents, King Joseph Bonaparte and others who were directed from afar by Napoleon. That is what Mr. Glover promises in his preface. Since he is a nonacademic military historian intent on telling a simple, dramatic story, it is understandable that he avoids dealing with the actions of Spanish armies and guerrillas.

Most positively, however, this is no book for scholars. It provides nothing new, even as military narrative (though it is news to me that Joseph had military ambitions). Naturally it barely mentions Joseph's valiant efforts to modernize Spanish institutions and society. It scorns his Spanish supporters, the *afrancesados*, though even Spanish historians, for example, Miguel Artola, now recognize that many were both talented and patriotic. There is no facing up to the fact that Wellington's victories served to restore Ferdinand VII, the most reactionary Spanish king since Philip II.

The author uses no Spanish sources. He seems oblivious to recent scholarship in whatever language. His newest specialized work in French is Geoffroy de Grandmaison's *L'Espagne et Napoléon* (1908–31). His French primary sources, all printed, are chosen haphazardly and used naively. He quotes from and tries to explain, for example, one of the most patently spurious of all bogus Napoleonic letters—to Murat, dated March 29, 1808. Intended apparently to shift some blame for the Spanish debacle to Murat, it was published by Las Cases in his *Mémorial de Sainte-Hélène* (1823). No original, draft, or even record of its dispatch or receipt has ever been found.

Whatever one's objections, however, the book is a success, considering Mr. Glover's announced intentions. It is saved by his skillful use of British sources, which he knows from his research for *Wellington's Peninsular Victories* (1963) and *Wellington as Military Commander* (1968). On the French side, Grandmaison, if outdated overall, serves Mr. Glover's purposes well. As straight narrative the work is generally valid and most readable. With the caution that the title is misleading, it may be recommended for undergraduates and military history buffs.

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RICARDO DE LA CIERVA. *Historia de la guerra civil española*. Volume 1, *Perspectivas y ante-*

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*cedentes, 1898–1936*. Madrid: Librería Editorial San Martín. 1969. Pp. xxx, 826.

Since he first became director of the "Unidad de Estudios sobre la Guerra de Espana" in the Spanish state ministry of information in 1966, Ricardo de la Cierva has become the leading figure in current Spanish Civil War studies. Scion of a distinguished upper-middle-class family, he lost both his father and his uncle in the war and in that sense has been closer to the conflict than most of the younger historians whose work has been conspicuous in the past decade. His study unit on the Civil War, which he largely relinquished to become director of the government's Editora Nacional in mid-1971, has enjoyed the full backing of the ministry of information and represents the first full-scale attempt made within Franco's Spain to study the Civil War on a systematic and relatively objective basis.

The major fruit of La Cierva's work is a projected three-volume history of the conflict, of which the present tome is the first (and to this date the only) volume. However, in addition to numerous short pieces on a myriad of topics, La Cierva has already published a lavishly illustrated and extensive two-volume synopsis of this project under the title *Historia ilustrada de la Guerra civil española* (Barcelona, 1971).

The present book in fact constitutes the most detailed one-volume account of the Second Spanish Republic ever written, since all but 145 of its 816 pages deal with the years 1931–36. This is also the first history of the Republic or Civil War to be preceded by an extensive historical introduction. The main theme is the politics of the Second Republic and its progressive deterioration into civic collapse and radical polarization. La Cierva writes presumably as a Franquist, but he is no propagandist. In part because of the support enjoyed, he has a greater fund of data at his disposal than any of his historiographic predecessors. To this is added a judicious analytical mind that avoids many of the pitfalls that have trapped earlier historians of both the Right and Left. La Cierva is sensitive and discriminating in his treatment of personalities, as in the analysis of relationships between political forces. His personal opinions are not overly obtrusive and the style is professional throughout. There is not overmuch analysis of social and economic issues, but such factors are treated to some extent in the areas where they are most pertinent, and the twenty-page section on the influence of international affairs in 1934–36 is one of the better treatments of that problem.

The two sectors of Spanish politics that receive major blame for the breakdown of the system are the revolutionary Left (a fairly obvious conclusion) and the moderate parliamentary Right of Gil Robles. The leader of the Catholic CEDA is thus treated rather more unfavorably than the virulently anticlerical Manuel Azaña, leader of the moderate Left. Since Azaña held power for a lengthy period and Gil Robles never did at all, this emphasis might be questioned, but it reflects a common trend in Spanish political attitudes (highly divergent on other issues) since 1936. Moreover, La Cierva is in general discreet and careful and not unmindful of the strong support Azaña's image still draws among the cultural and historiographic intelligentsia.

Five parts deserve particular mention. The first is La Cierva's one-hundred-page section on the Azaña regime of 1931–33, which rivals Gabriel Jackson's treatment of that topic. The second is the equally lengthy and indeed magisterial account of Spain's "October revolution" of 1934. La Cierva gives the fullest and best treatment ever published of both the Catalan and Asturian phases of this insurrection. The seventy-page section on the "Birth and development of Spanish Fascisms [*sic*]" is proportionately the best thing written on the fascistic variants in Spanish politics under the Republic.

The most original single section is the chapter "The Genesis of the Popular Front." La Cierva provides the clearest and fullest account we have of the origins and development of an enterprise that began as an alliance between the middle-class Left and the moderate Socialists only to give ground steadily before the revolutionary Socialists and Communists. The book concludes with a detailed narrative of the military conspiracy of 1936. This final section does not introduce any major novelties but adds a number of new details.

The Spanish Civil War is still too near contemporary politics for most historical accounts to be accepted with complete equanimity. Since La Cierva has been at pains to be fair to

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the moderate Left, he was strongly denounced by some elements of the extreme Right in Spain after this book was published. The book has been received with even greater hostility by much of the Left. However that may be, it constitutes a major contribution to the historiography of the Spanish Republic that must be carefully studied by all future scholars.

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ARXIU VIDAL I BARRAQUER. *Església i estat durant la segona República espanyola, 1931–1936.* Volume 1, *14 d'abril–30 d'octubre de 1931.* In two parts. Edited by M. BATLLORI and V. M. ARBELOA. (Scripta et Documenta, 20 and 21.) [Montserrat:] Monestir de Montserrat. 1971. Pp. xiv, 280; 284–558.

The Catholic Church becomes a critical constitutional issue each time there is a political change-over in Spain. It seems no mere coincidence that in the ebb of the present regime, the Monastery of Montserrat (opposed to Franco) is publishing the papers of Francisco Cardinal Vidal i Barraquer for the period of the Second Republic. A complex figure, Vidal attempted to detach the Church from Monarchists and Carlists and to negotiate in good faith a "formula of conciliation" with the republic. Despite his failure, he was one of only two hierarchs who did not sign the pro-Franco Collective Letter of the Bishops in 1937: denied permission to return to Spain, he died in exile in 1943. If proof be needed, his career serves as a further instance of the pluralism of the Spanish Catholic Church.

This first series of documents covers the period from the proclamation of the Republic to the enactment of the anticlerical Articles of the Constitution; the text is in both Catalan and Castilian, with footnotes containing extensive data and bibliography. As the senior cardinal in Spain (by date of appointment), Vidal served as the primary Church negotiator in the absence of the primate. Despite Cardinal Vidal's good will toward the republic and the nuncio's complete support, the cardinal failed. Among other causes, it was difficult for him, as a Catalan, to lead the entire Spanish Church; not only was he head of the Church in Catalonia, as archbishop of Tarragona, but he had publicly defended his region against the Primo dictatorship. Another obstacle was Vidal's idealistic opposition to "unipersonal" direction of the Spanish Church. He proposed instead "unidad pero no conformidad," a decentralization intended to demonstrate to a skeptical Cortes the continuing vitality of Catholicism in Spain. But he found no real support for this, nor for his effort to detach the hierarchy's pastoral role from papal diplomacy. Church policy remained largely the work of the Vatican secretary of state and the nuncio, thus substantiating the classical anticlerical charges against the authoritarian, ultramontane Church. Laymen were excluded, asked only for support through letters and rallies. But "Catholic" deputies definitely did make policy, primarily for political reasons. "Consciously or unconsciously," protested a Church official, "they aspire to use the Church as an instrument to destroy the Republic." This dimension of political activism is missing as there are no reports of Vidal's contacts with "Catholic" deputies. The editors state that they have published all such material in the archive; we must conclude that Vidal dealt directly with only one "Catholic," President Alcalá Zamora. Documents also report Vidal's contacts with two cabinet members, Fernando de los Ríos and Lerroux, who demanded guarantees, specifically the resignation of Cardinal Segura as a sign that the Church disavowed his pro-Monarchist policies. The nuncio and Vidal urged the Vatican to act, but it delayed until the eve of the Cortes debates by which time the Segura incident had exacerbated partisan feelings on both sides. It also diverted the negotiations from resolution of the fundamental substantive differences between the government and the hierarchy concerning the Church in the republic. Ironically the Cortes' enactment of anticlerical measures vindicated Segura's intransigence and discredited the rapprochement of the nuncio and Cardinal Vidal.

JOAN CONNELLY ULLMAN
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ERICH THOMSEN. *Deutsche Besatzungspolitik in Dänemark 1940–1945.* (Studien zur modernen Geschichte, Number 4.) [Dusseldorf:] Bertelsmann Universitätsverlag. 1971. Pp. 277. DM 34.

ALAN S. MILWARD. *The Fascist Economy in Nor-*

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*way*. New York: Oxford University Press. 1972. Pp. x, 317. $17.00.

Historians interested in Scandinavia, in Germany, or in World War II should be pleased with both of these useful scholarly works although each has limitations. Both books are well organized, well written, and based almost entirely on primary sources. Both present the events of 1940–45 in Denmark and Norway mainly—but quite dispassionately—from the German angle (or angles). Neither book is concerned with military operations.

The work by Thomsen is particularly gratifying, presenting the political history of the German occupation of Denmark systematically, sensitively, comprehensively, and in considerable detail. Emphasis is on the thoughts, the plans, the comings and goings, the day-to-day (and sometimes hour-by-hour) communications and actions of the various German authorities in Copenhagen and Berlin. Thomsen is obviously sympathetic to the situations and the views of the German plenipotentiaries who, operating under the ministry of foreign affairs, managed the German relations with and exploitation of Denmark with considerable finesse, some humanity, and never-failing pragmatism. He claims that, on the whole, they succeeded in keeping Denmark producing for the Germans until the end of the war, warding off policies and actions which might have jeopardized that production—although there were trying times (examined in detail) when Danish saboteurs, the Wehrmacht, the SS, ideological Nazis either German or Danish, or Hitler personally brought on crises. The constant infighting for power among high German personalities and agencies is the core subject of the book and is well described although Thomsen's sympathies occasionally shade the presentation ever so lightly. The personalities of the leading German protagonists emerge well rounded and nuanced. By contrast the Danes and the Danish side of developments are left very flat, possibly on the premise that Danish historians have already ably presented them. Thomsen gives good marks to Danes like Scavenius who pragmatically collaborated with the German authorities while preserving Danish institutions and most Danes. The Danish Nazis are derided —the Danish Resistance emerges as significant only because it sometimes resulted in greater German repression.

The work by Milward is much more limited. It is ninety-nine per cent economics if one includes the extensive tables on production, rationing, etc. And as a presentation of the economic problems in occupied Norway and of the German-Norwegian economic relationship it is excellent. Milward also contends that Fascist ideology (both German and Norwegian) influenced developments. There, however, he fails. Although there were some national-socialist-type dreams, especially early in the occupation, they were brutally shunted aside for military and economic reasons. Norway did not fit easily into the *grossraumwirtschaft* of a German-dominated Europe at war with the rest of the world and, pure nordic stock notwithstanding, was ruthlessly exploited. The word Fascist in the title is misleading. What Milward details is the story of the Norwegian war economy, 1940–45, mainly as it served or (increasingly) failed to serve German needs. Milward, like Thomsen, also makes it clear that the German occupiers, although relatively "efficient" were continuously at cross purposes with each other.

HEINZ E. ELLERSIECK
*California Institute of Technology*

CLAUDE NORDMANN. *Grandeur et liberté de la Suède (1660–1792)*. (Publications de la Faculté des Lettres et Sciences Humaines de Paris-Sorbonne. Series "Recherches," Number 63. Travaux du Centre de Recherches sur la Civilisation de l'Europe Moderne, Number 9.) Louvain: Éditions Nauwelaerts. 1971. Pp. 551. 1,160 fr. B.

In the tradition of recent French historical work Nordmann's Swedish history emphasizes demographic and social aspects. It describes the changing agrarian organization and the growth of industrial production, deals with economic and cultural conditions, and, unlike Michael Roberts's recent Swedish history from 1523 to 1611, assigns a less conspicuous part to political, constitutional, military, and ecclesiastical affairs. Two-thirds of the book are devoted to the first 110 years, one-third to the 22-year reign of Gustavus III.

The presentation is well balanced. It shows the dynamism of Sweden's evolution, emphasizing the spurts made in the age of Charles XI

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(which witnessed the *Güterreduktion* in Sweden and Livonia) and that of Gustavus III (which witnessed the transition from early enlightenment to traditional Vasa absolutism). Least satisfactory is the part on Charles XII, which, understandably, deals mainly with diplomatic and military affairs and, being too involved, might have benefited from a more summary presentation.

Economic aspects are comprehensively treated and are particularly enlightening because the author pays attention not only to agriculture, industry, and trade but also to finance. He shows how economic policies made possible Sweden's ambitious military undertakings, but also how these, in turn, influenced the development of the economic forces and brought changes in the administrative apparatus and the social services. Although pointing out that considerable social mobility existed, the author, through careful analysis, shows that actually nobility and Church were successful in maintaining many of their privileges and in defending themselves against the encroachments of their rights by the monarchy as well as against the challenge of a rising third estate.

Two interesting chapters are devoted to Sweden's cultural activities—science, history, literature, and religion, with special attention to the mysticism and new religiosity of the eighteenth century. These, Nordmann claims, were a result of anxieties in an age of uncertainty. In describing cultural affairs, the author demonstrates that Sweden increasingly participated in the great movements and the intellectual currents of the whole European continent. The reader comes to realize that, as for so many nations, the periods of Sweden's cultural advances did not coincide with those of her political greatness.

There are some details that can be challenged in Nordmann's presentation: Have contemporaries and historians, as Nordmann indicates, judged the policies of Charles XII's adviser, Baron von Goertz, too harshly? Was there a paradox in Gustavus III's policies because they led from "much vaunted constitutional liberty" to absolutism? Did the *Güterreduktion* yield as much revenue as the author's statistics indicate (the Estonian historian Piiremäe came to different results)? Did the China imports given at eleven million riksdaler really provide the households of no more than 120 families? Furthermore, we may ask if, in connection with Nordmann's discussion of social mobility, more accent should not have been put on the changes within the bureaucracy and the growing role of academically trained officials. Also, the bibliography, though extensive, is really not comprehensive. But within the limits Nordmann has set for a history of this kind, it is an outstanding contribution, modern in concept and equally useful for the general reader in history and for the scholar.

WALTHER KIRCHNER
*Princeton, New Jersey*

REINHARD HÖHN. *Scharnhorsts Vermächtnis.* 2d ed.; Frankfurt am Main: Bernard & Graefe Verlag. 1972. Pp. 387. DM 16.

This book has an unusual history. In 1944 Professor Höhn published a long analysis of ideas on war between the Frederician and Napoleonic eras with the title *Revolution, Heer, Kriegsbild.* Apparently much of the edition was destroyed in an air raid, though copies of the book can be found in libraries and occasionally turn up in secondhand bookshops. In 1952 the author brought out a revised version of a part of this work, *Scharnhorsts Vermächtnis,* which has now been republished. The book, which lacks much of the useful background material of its predecessor, treats the impact of the French Revolution on German military opinion, Scharnhorst's efforts at reform before and after 1806 and his attempt to develop an intellectual elite—the general staff—as the only type of leadership adequate to modern war. It ends with a discussion of Scharnhorst's "legacy" to the present: in total war the politically neutral officer must give way to the ideologically committed soldier—a thesis that owes less to Scharnhorst than to the beliefs of the author, who before 1945 was a senior SS officer as well as a teacher at the University of Berlin.

It would not be difficult to dismiss a work of such provenance; but in fact the author knows a great deal about his subject, he worked in the relevant archives, and his argument rests on extensive quotations, often from documents that no longer exist. The interest of these texts is

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considerable; unfortunately they are not always accurate, as became evident some years ago when I compared quotations with documents that had escaped destruction (see my *Yorck and the Era of Prussian Reform* [1966], pp. 283–84), and in the process of reducing the original work to the present study further errors have crept in.

No archival research is needed to discover the book's methodological flaws. Höhn writes about the interaction of theory and reality. But he scarcely explains how Scharnhorst's ideas developed, how he taught men to shed tradition, and how new insights were translated into policy. How can he, since he does not, for instance, distinguish between Scharnhorst's published writings, confidential reports, and family letters? In Höhn's interpretation ideas take on an absolute quality, whatever the circumstances of their formulation and expression. The result is not intellectual history but an ideological exercise that reconstructs men's attitudes from scraps of barely differentiated evidence. Such an approach may isolate relevant issues, as it does in this book, and define them with unrealistic sharpness; but at the end one is bound to say: that's what it was about, but that's not the way it was.

PETER PARET
*Stanford University*

THEODORE S. HAMEROW. *The Social Foundations of German Unification, 1858–1871: Struggles and Accomplishments.* Princeton: Princeton University Press. 1972. Pp. vii, 456. $15.50.

The gap in time between the publication of the first volume of Theodore S. Hamerow's *Social Foundations of German Unification, 1858–1871: Ideas and Institutions* (1969) and its sequel *Struggles and Accomplishments* should not prevent the two from being regarded as a single work. As such, it is one of the most important and interesting studies of nineteenth-century German history to have appeared in English for some years. The first volume was an example of history with the politics left out. The second volume now puts the politics in again. Thus the slight uneasiness left behind after reading the previous book is dissipated and one no longer has the sense that what is being discussed is somehow unconnected with the great events to which it provides a background. The detail of the social and economic structure of Germany in which the first volume is so rich now falls into place and perspective.

The politics with which this book is concerned are, of course, far from the statecraft that is the central theme of almost all previous books on German unification. There is very little about Bismarck's diplomacy or the military preparation and conduct of the wars of 1864, 1866, and 1870; and, even when compared with Helmut Böhme's *Deutschlands Weg zur Grossmacht* (1966), in which some of the arguments are not dissimilar, there is not a great deal about the actual execution of governmental policy. Instead, Professor Hamerow builds up a cumulative picture of the irresistable movement for economic change in Germany and its impact on the policies of the German states. The study is based mainly on printed sources and, indeed, demonstrates what a wealth of material may be found without going to the unpublished archives, which can no longer contain many secrets. One would have liked some maps, a fuller index, and perhaps a more critical and elaborately organized bibliography; and the practice of grouping the references in long footnotes every few pages instead of noting each reference separately is sometimes confusing. In general, however, the book is notably easy to read and well produced.

Both volumes concentrate largely on Prussia. This follows logically from the argument. Since economic development in Prussia was more extensive and more rapid than elsewhere in Germany, it was in Prussia that the pressures for change from the representatives of economic interests were strongest, and for this reason, too, it was on Prussia that the hopes of similar groups in the other German states were centered. Hamerow shows convincingly that the real demands of the new industrial class were less concerned with constitutional developments than with the liberation of trade, the mobility of labor, and, above all, the removal of restrictions on credit so as to create conditions in which joint-stock companies could be formed and capital raised for new industrial enterprises. The book is not concerned with possible alternative solutions to the German question. It does not claim to analyze Austrian policy very deeply or to explore the problems

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confronting Prussia in the territories annexed after the war of 1866 (the subject of a forthcoming book by a young British historian, Dr. Nicholas Hope). But this study shows the nature of some of the forces that made Bismarck's Germany what it was and gives an account of many of the underlying reasons for Prussian success.

In this account the constitutional conflict in Prussia was almost irrelevant. Some of the economic changes demanded by the Liberals had been made before it began; and even the political changes conceded by Bismarck in the spring of 1866 and in the constitution of the North German Confederation were, on this view, less important than the economic changes that the Prussian government had already accepted. As Hamerow puts it, "The political reconciliation of the years after 1866 was foreshadowed by the economic rapprochement of the years before 1866. The same legislators who thundered against the unconstitutionality of Bismarck's rule supported him in the struggle for commercial hegemony in Central Europe. . . . The uncompromising rhetoric of civic ideology disguised the reality of a common economic objective" (p. 220).

If—and the parallel was, as Hamerow shows, never far from the minds of liberal German publicists—Bismarck had been Cavour, then there might have been in northern Germany as in northern Italy a liberal political system as well as a liberal economic one, but, as the *Jahrbücher für Gesellschafts- und Staatswissenschaften* (1866), quoted by Hamerow (p. 287), pointed out, "Count Cavour was a liberal statesman who attained liberal ends with the means of liberalism, while Count Bismarck made liberal ideas and energies subservient to conservative ends." Perhaps, after all, politics did in the long run take precedence over economics.

JAMES JOLL
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ANDREW R. CARLSON. *Anarchism in Germany.* Volume 1, *The Early Movement.* Metuchen, N.J.: Scarecrow Press. 1972. Pp. vi, 448. $12.50.

If a diligent search for source material were all that historiography requires, this would be a praiseworthy book. The author has gone through a large number of police archives and other public depositories in Germany for information on the personality, actions, and fate of anarchist leaders. To be sure, he might have supplemented this material by also investigating the archives of neighboring countries from which German anarchists operated, but to fault him for lack of this additional effort would be unfair. Carlson also has used secondary sources with great thoroughness.

But in analysis the book is inadequate. Sometimes an attempt at analysis stops just where the reader would like to know more; for instance, after having explained Richard Wagner's anarchist inclinations the author simply says: "Wagner, of course, cannot properly be called an anarchist" (p. 19). Probably not, but what were the counteracting trends in his thinking? The Haymarket affair in Chicago (1886) is just barely mentioned, although German anarchists were almost exclusively involved in this tragedy and it must have had an impact on the fratricidal struggle between the upholders of the Bakuninist tradition, primarily John Most and Victor Dave, and the followers of Kropotkin, Joseph Peukert, Erich Rinke, and their friends—one of Carlson's major topics. One of the Lassallean leaders, Wilhelm Hasselmann, went over to the anarchists; the author does not once raise the question of how a Lassallean can become an anarchist, although there is no greater antagonism than between Lassalleanism, with its emphasis on suffrage, and the anarchist contempt for peaceful political action. Most important: How could anarchists, being devoted to a society free from force, develop the philosophy of the propaganda of the deed? Specifically, how could a man like John Neve, a morally sensitive person of superior human qualities, smuggle dynamite and acid into Germany to be used in a manner that would make innocents suffer with those guilty of oppression and even be greatly interested in a device for causing instant death by plunging a poisoned needle into a person's body?

Although the author overrates the importance of German anarchism—see the rather ridiculous statement: "If Marx had not been goaded by the anarchists, it is doubtful if he would have done much writing" (p. 397)—he does not believe that the anarchists could have changed German society. Neither does he be-

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lieve in the parliamentary strategy of the Social Democrats; yet he concedes that to this strategy there was no alternative. What course of action, then, would Carlson have wanted the German working class to take? His argument has no point, aside from the factual error that Social Democratic policy in the Reichstag led to no results.

For the second volume of this book the author or the publisher should arrange for some efficient proofreading; the first volume beats any record in printing mistakes, including misspelling of names.

CARL LANDAUER
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*Akten zur deutschen auswärtigen Politik, 1918–1945, aus dem Archiv des Auswärtigen Amts.* Series C: *1933–1937. Das Dritte Reich: Die ersten Jahre.* Volume 1. Part 1, *30. Januar bis 15. Mai 1933;* part 2, *16. Mai bis 14. Oktober 1933.* Göttingen: Vandenhoeck & Ruprecht. 1971. Pp. lxxviii, 434; 435–979. DM 32.50 each.

The original ambitious plan of the United States, British, and French governments to instruct a group of editors with the publication of a collection of German foreign ministry documents for the period 1918 to 1945 was in practice severely curtailed. Series D, originally designed to cover 1937 to 1945, was terminated with December 1941; it appeared in thirteen volumes in English with only the first seven also published in Germany. Series C was to deal in six volumes with the first years of National Socialist rule. The first of these appeared in 1957. Others were published subsequently but the sixth is not yet out. As the originals were returned to the Germans, the government of the Federal Republic agreed to assume responsibility for continuing the project with the other three powers remaining represented. The new system was to provide for the preparation and publication of the remaining series on a basis essentially similar in concept to that orginally designed. Series A, covering 1918 to 1925, has been deferred. Series B, which begins with the signing of the Locarno agreements on December 1, 1925, is in the process of publication (in German only) and is making steady progress; volume 1 appeared in 1966, and the editors have reached volume 5. The documents for December 1941 to 1945 are being issued in a series E of which two volumes have been brought out. The German editors, furthermore, took on the preparation of German editions of volumes 8 through 13 of series D, all of which have been published, and of the volumes of series C of which this book is the first.

In the German edition the same documents appear and in the same sequence as in the English (see my comments in the *Journal of Modern History*, 31 [1959]: 63–64). Certain changes have been made, most of them improving the volume. A number of minor errors has been corrected, and some film serial numbers that indicate archival origin, which had been previously omitted, are now included in the listing of serials. The notes have been amplified in many instances, and there are additional notes specifying dates and identifying persons. A name index is a welcome addition. Cross-references have been provided to important new publications, especially of French diplomatic documents and of further German documents on the concordat negotiations. The papers of Polish ambassador Josef Lipski were inexplicably omitted from this practice. Some errors and gaps could have been taken care of by further searching; thus the name missing as illegible in document 166 could have been identified as Diels from another copy on serial 4602, frame E 189416, and the error in the date of document number 405, note 5, corrected from serial 3015, frame 597727. Such examples only show that there are limits to even the most careful technical editorial work. More important is the unfortunate failure of the German technical editor Franz Knipping to devise some simple mechanism, such as starring the document number or underlining the serial number, that would have alerted the reader to the fact that the document reproduced is one of the relative few not taken from the foreign ministry archives. Why the editors imagine that binding the volume in two parts, with the analytical list of documents in one and the appendixes and index in the other, should make the work easier to use is unclear. This is one departure from the English edition that one hopes will be abandoned.

A generation of students whose internationalistic sentiments take the form of insisting

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that all foreigners learn English will continue to depend on the English language edition. The translations are quite reliable and the editorial errors few in number. Scholars will be pleased to have a German edition in which small but useful corrections and improvements have been made. From Hitler's succession to power in January to Germany's departure from the League of Nations and the Disarmament Conference in October 1933, here is the basic record.

GERHARD L. WEINBERG
*University of Michigan*

MIRON CONSTANTINESCU *et al. Unification of the Romanian National State: The Union of Transylvania with Old Romania.* Under the editorship of MIRON CONSTANTINESCU and ŞTEFAN PASCU. (Bibliotheca Historica Romaniae. Monographs, Number 7.) Bucharest: Publishing House of the Academy of the Socialist Republic of Romania. 1971. Pp. 367. Lei 30.

The recent outpour of scholarly studies on the history of Transylvania is only marginally connected with the revival of the cult of Dracula. It is also only marginally related to the needs of historical scholarship since those needs have been well served by the same historians who have produced the latest volume on the union of Transylvania with old Romania.

The proliferation of works on Transylvania is based on political considerations not dissimilar to those that resulted in the glutting of the market with tomes of varying degrees of learning and wisdom since the beginning of the twentieth century. The book under review, like its predecessors, is ultimately designed to prove the historic validity of Romanian rights to Transylvania. The case is well stated.

The "premises of the political unity of the Romanians," nationalism and numerical preponderance in Transylvania, are carefully developed by Ştefan Pascu, a distinguished historian and the rector of the University of Cluj. He also contributes a succinct statement on the "Memorandum" movement, the abortive formulation of Romanian claims to national equality in Transylvania. The post-Memorandist evolution of the Romanian national movement is discussed both in terms of internal conditions and relevant external factors by C. Nuţu and I. Gheorghiu. Three specialized studies on the contributions made by the peasantry, the working class, and the intellectuals to the "struggle" for the union of Transylvania with Romania are provided by V. Popeangă, I. Kovács, A. Porţeanu, and V. Curticăpeanu. Miron Constantinescu, a sociologist, a historian, and the foremost exponent of contemporary official ideology, is the author of the thoughtful chapter on the act of union of December 1, 1918, and also of the less learned but equally significant afterword. Sandwiched in between Constantinescu's contributions are a conventional summation by Ion Opera and the excellent if altogether too brief chapter by L. Bányai and C. Göllner, "The Position of the Nationalities Toward the Union."

All aspects of the process of unification are thus covered with an eye to restating the Romanian case and refuting, implicitly or explicitly, "revisionist" theories on Romania's historical rights to Transylvania. Constantinescu's afterword carries all the arguments to logical conclusion in pronouncing the total fulfillment of the historic aspirations of all inhabitants of Transylvania under the rule of the Romanian Communist party in the Socialist Republic of Romania.

Although minor exceptions may be taken with certain specific historical arguments, no serious criticism may be levied on scholarly grounds. The main questions fall in the rubric of the politic. As historians, and in the case of Miron Constantinescu also as a man of politics, the authors must be well aware of the fact that historical evidence and political expediency are not necessarily compatible. The Vienna Diktat of 1940, like the restitution of northern Transylvania to Romania in 1945, was based on political and not on historical justification. The historical case for Transylvania's union with Romania in 1918 and reunion in 1945 is clear but no amount of repetition of well-known arguments will consolidate Romania's hold on Transylvania should the external political circumstances be adverse to Romania's interests.

It is unfortunate that comparable scholarly studies cannot be produced on the history of other Romanian-inhabited territories currently outside the Romanian body politic. Historical studies on Bessarabia, Bukovina, and the Moldavian Socialist Republic as such, whether written by Romanian scholars, Russian schol-

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ars, or scholars of other nationalities, are needed much more urgently than yet another volume on the history of Transylvania.

STEPHEN FISCHER-GALAȚI
*University of Colorado*

DOUGLAS DAKIN. *The Unification of Greece, 1770–1923*. New York: St. Martin's Press. 1972. Pp. xiv, 344. $11.50.

The author defines his objective as being "to do for modern Greece what others have done for modern Italy—to tell the story of how unification was achieved" (p. viii). He interprets his task broadly, going beyond purely diplomatic history to provide the domestic background of Greek foreign policy. The end result might be fairly described as a general history of Greece with emphasis on foreign affairs.

He begins with a perceptive analysis of the dynamics of the Greek world at the end of the eighteenth century. Then he proceeds to the abortive uprisings of 1770–1814, emphasizing the roles of the protomartyr Rhigas and of Ali Pasha of Yanina. The substance of the book comprises the study of what the author terms the four wars of independence: that of 1821–33; the struggle for Crete (1833–97); the struggle for Macedonia (1897–1908); and the Asia Minor war culminating in the 1922 disaster, which the author interprets as the culmination of a successful unification. These national undertakings are analyzed in depth by relating them to Greek resources, to domestic political currents, to great power interventions, and to the Great Idea in its different forms and the Church-state conflicts to which it gave rise. In conclusion the author contrasts the Greek and Italian unification processes, emphasizing how much less time and cost was involved in the Italian case. This he attributes primarily to great power policies that worked in favor of the Italians and against the Greeks and to geopolitical considerations such as the numerical weakness of the Greeks and their fragmentation by Slavic, Albanian, and Turkish ethnic enclaves.

Finally the study is supplemented with several very useful appendixes, including "Governments of Greece 1833–1924," "Elections and Parties in Greece 1844–1924," "Population, Emigration, and Occupations," and "Public Finance and Foreign Trade."

L. S. STAVRIANOS
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JOSEPH T. FUHRMANN *et al. Essays on Russian Intellectual History*. Foreword by JAMES P. HART. Introduction by SIDNEY MONAS. Edited by LEON BORDEN BLAIR. (The Walter Prescott Webb Memorial Lectures, Number 5.) Austin: University of Texas Press for the University of Texas at Arlington. 1971. Pp. 123. $5.00.

To mark the manifold interests of the late Walter Prescott Webb the fifth memorial lectures were devoted to aspects of Russian intellectual history. Three studies and an introductory essay were the results of the lecture series and are published in the nicely produced book under review.

In modern times Russian intellectual life always took place within the more or less narrow limits set by official censorship. It is therefore appropriate that in the last essay—thematically it should have been first—Professor Twarog presents a brief sketch of literary censorship from the reign of Catherine II to the present. The bulk of the essay focuses on the Imperial period, since Soviet censorship is both less documented and better known in its immediate consequences. It is a useful survey, albeit a trifle superficial, that points out the qualitative difference between "negative" censorship, as practiced before 1917, and the "positive" controls of the Soviet system. One might have wished a still greater stress on this difference to avoid the impression of a "natural continuity" of censorship practices throughout modern Russian history. Such an impression is reinforced by Twarog's failure to note that censorship, as conceived and practiced in Imperial Russia, was an all-European phenomenon (with only some geographic and chronological exceptions) in both the eighteenth and nineteenth centuries.

The interesting question that Twarog does not address is why censorship in Russia had such a provocative and powerful role in shaping the intelligentsia's self-image and thinking. Perhaps it is because of this very fact that not all manifestations of Russian intellectual life may be readily ascribed to the intelligentsia. In the realms of religion, philosophy, and espe-

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cially science and scholarship, much was created in the open, in relative freedom from government oppression, and did not become a part of the intelligentsia's concerns; it has for this reason been neglected by historians. The two remaining studies in the present volume help to correct that perspective. It is therefore unfortunate that Professor Monas, in an otherwise thoughtful and suggestive introduction, creates the impression of trying to fit them, too, into the traditional concept of the intelligentsia's role in Russian intellectual history.

Edward Bock gives a workmanlike summary of the political and social views of Vladimir Soloviev, the great religious philosopher of the late nineteenth century. While his presentation is on the whole accurate and useful for an understanding of the ideals of Vladimir Soloviev (which Professor Monas quite suggestively likens to those of the later T. S. Eliot) it does not deal adequately with Soloviev's overpowering concern for the moral aspects of power (as exemplified by his best known work, "Three Conversations"). In this concern he was in the tradition of the intelligentsia, but the answers he suggested were very different from theirs, so that he remained a marginal figure. Yet he was a figure of great influence, whose impact needs further elucidation. His solution—the necessity, for both the individual and the group, of a return to institutionalized but unpolitical religion for a *total* moral solution—appealed to many in the twentieth century and was a crucial factor in the transformation and fate of both the Russian intelligentsia and culture. (Solzhenitsyn, for example, cannot be understood without reference to this transformation.)

The volume's most substantial essay, qualitatively and quantitatively, is Joseph T. Fuhrmann's intellectual biography of the "Ukrainian Socrates," Grigorii Skovoroda (with an excellent *bibliographie raisonnée*). It is also a valuable study since there is virtually nothing in English on Skovoroda, who was a curious as well as exemplary figure of Russian intellectual development at the end of the eighteenth century. The article might have been even more useful if Fuhrmann had tried to situate Skovoroda more precisely in the intellectual environment—philosophic and religious—of his time. For the wandering sage was much less of an "odd man out"—except in a biographical sense—than is implied here. His religious and philosophical seekings (and the sources for his solutions or rather nonsolutions) were part and parcel of the broader cultural scene of the late eighteenth and early nineteenth centuries; seekings of which Radishchev, Novikov, and the Turgenev circle are only the best known and more secularized examples.

The essays in this volume point to the broader perspective in which Russian intellectual life should be viewed if its richness and dynamism are to be fully appreciated. This slender volume has made a yeoman's contribution in this direction. Its limitations and defects weigh lightly in comparison.

MARC RAEFF
*Columbia University*

S. FREDERICK STARR. *Decentralization and Self-Government in Russia, 1830–1870.* Princeton: Princeton University Press. 1972. Pp. xiii, 386. Cloth $15.00, paper $8.95.

A happy exception to the general rule governing first books, the substance here is impressive and only the title remains prosaic. Professor Starr has freed Russian local government from the fetters of purely descriptive administrative history. A strong dose of bureaucratic and interest-group politics has restored the subject to its rightful place as a central arena in the struggle over the welfare and security of the Russian people. Following a lively introduction on the "undergoverned" condition of Russia's provinces, Starr argues that Russian intellectuals of every stripe recognized the need for reform long before the Crimean War. Although some sought models in the experience of Western Europe, especially as Tocqueville defined it, and others turned to their own past reviving regional or federative memories, they were all united by a naive faith in the efficacy of structural change. By contrast, the shock administered to the civil service by military defeat and financial chaos pushed the government toward institutional change. Bitter bureaucratic infighting between interest groups aligned along horizontal (ministerial) and vertical (center versus periphery) axes determined the compromise character of the zemstvo and provincial governor reforms. As a result the vital functional distinctions between "the up-

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graded local administration and the newly founded societal agencies" were never made clear. Balancing this negative feature the author acknowledges that "the Russian state underwent a major constitutional crisis without bloodshed or the use of major force," thus demonstrating "a greater vitality than might generally be ascribed to it" (pp. 290–91). But the absence of trained personnel, the growing fears over internal security, and the re-emergence of Russia on the international scene as a major power led to measures that crippled both self-government and decentralized administration. In their place re-emerged the old Russian tradition that local agencies should serve as the arm of the central government. (Here Starr speculates that a parallel response by Communist party leadership to similar historical pressures helps explain the checkered career of the local Soviets.) Nevertheless, a residue of real social accomplishments remained at the provincial level. Written in an engaging style and based on a wide range of archival sources, this study succeeds admirably in combining a firm grasp of empirical data with valuable insight provided by the growing theoretical literature on bureaucracy and decentralization of political power. Thus, the comparisons Starr makes with nineteenth-century France and present-day underdeveloped societies become valid and illuminating. In sum, this is an example of the "new history" at its best.

ALFRED J. RIEBER
*University of Pennsylvania*

RAYMOND T. MCNALLY. *Chaadayev and His Friends: An Intellectual History of Peter Chaadayev and His Russian Contemporaries.* Tallahassee, Fla.: Diplomatic Press. 1971. Pp. 315. $15.00.

Professor Raymond McNally is a specialist on Chaadayev. He has already given us a scholarly edition and a translation into English of Chaadayev's major writings as well as several significant articles on that crucially important and still rather enigmatic Russian thinker. The present book moves from "Chaadayev's Life" and his intellectual battles "Against the Slavophiles" and "Against Nationalistic Exclusiveness" to the issues of Chaadayev and "The Christian Churches," "Influences on Chaadayev," and Chaadayev "Revolutionary or Reformer?" There is a concluding "Final Evaluation," and the text is followed by a very rich and valuable section of over seventy pages devoted to source materials and bibliography, and by an index. F. A. Moller's famous oil painting of Chaadayev gazes at the entire enterprise.

Professor McNally's book is based on the latest research and information, found also in part in his articles. It is to McNally that we owe a better understanding of the evolution of Chaadayev's thought on Peter the Great or of the extent and intensity of Chaadayev's relationship to the Slavophiles. Especially rewarding is the fifth chapter, "Influences on Chaadayev," in which the author reconstructs Chaadayev's two libraries—the second of which the author himself has largely recovered for scholarship—and deals with the problem of influences, on the basis of these books and Chaadayev's numerous notes in their margins.

Yet in spite of its learning and interest the study is disappointing. Although not adverse occasionally to even sweeping generalization, McNally stays most of the time very close to his sources. In the preface he writes about his intention "to complement rather than supplement Quenet's fine work" (p. 3), and his book does become in large part just such a complement—fragmentary, uneven, and at times difficult to follow. Much worse is the matter of presentation. The Diplomatic Press provided a beautiful binding and clear print but apparently no editing or proofreading. One has to struggle with a text where "did not" means "did" (pp. 96–97), where "reverse" means "reverence" (p. 89), where whole sentences are garbled (as on p. 41), where Kukolnik is hidden under the name of Sukolnik (p. 30 and in the index, p. 314), where Sophia Meshcherskaia's dates are given as 1715–1848 (p. 38), where after a searching investigation of the relationship between Chaadayev and Ivan Kireevsky the latter is allowed to live four extra years (p. 92), and on, and on. The handling of the Russian in transliteration, and very occasionally even in translation, becomes sheer disaster.

The work of our leading researcher on Chaadayev deserves a much better presentation.

NICHOLAS V. RIASANOVSKY
*University of California, Berkeley*

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ANDREW D. KALMYKOW. *Memoirs of a Russian Diplomat: Outposts of the Empire, 1893–1917.* Edited by ALEXANDRA KALMYKOW. (Yale Russian and East European Studies, 10.) New Haven: Yale University Press. 1971. Pp. xv, 290. $12.50.

Alexandra Kalmykow, a professor of Russian at Monmouth College, has prepared for the press the memoirs of her father (1870–1941). They make fascinating reading, but they do not provide much new information. Professor Firuz Kazemzadeh has written a pithy two-page foreword; as a specialist on Russo-Persian relations he finds the pages (30–99) on Kalmykow's stay in Persia the most interesting. As Professor Kalmykow adds in her preface, her father's Bangkok service was also significant, since he originated the idea of securing French withdrawal from a Siamese port through cession of the region of Angkor to the French protectorate of Cambodia, which did much to make possible present-day Thai independence, and as consul in Üsküb (Skoplje) during the First Balkan War (1912) he may have averted a massacre of Christians there. The longest chapter, much of which is devoted to Kalmykow's assignment to escort Emir Seid Abdul Ahad of Bokhara on a state visit to St. Petersburg, contains a remarkable picture of the government of the little Oriental state that survived in the middle of Russia's Central Asian dominions. Kalmykow's casual remarks on Russia's domestic affairs were not profound: of Social Democrats and Socialist Revolutionaries, he thought "neither party was revolutionary at all" (p. 147); his evaluation of Stolypin (p. 178) is dubious; his story that Izvolski saved Rosa Luxemburg from judicial murder in Warsaw in 1906 (pp. 177–78) may be partly true but in specifics cannot be reconciled with Peter Nettl's biography of her.

None of these details seems as interesting as the self-portrayal of Kalmykow: a diplomat of Imperial Russia who had had an excellent Orientalist education, knew fourteen languages (his daughter reports), and approached Persians and Siamese without a trace of condescension; disliked war from childhood and had no sympathy whatever for adventurist posturing in foreign affairs; had no apparent political commitments to Left or Right but yet coupled patriotism with respect for all human beings and cultures; was gentle, never cruel or cowardly, and appreciated life's amenities while able to dispense with them without complaint. Though his account makes clear that he was not unique in these respects among his fellow diplomats, Russian and others, he still must have been an admirable person. His account skips from 1916 to 1919 without mention of the revolution and ends abruptly on his escape from the fire of Smyrna in 1922. The book does not tell where he spent the last nineteen years of his life.

DONALD W. TREADGOLD
*University of Washington*

ANATOLE G. MAZOUR. *The Writing of History in the Soviet Union.* (Hoover Institution Publications 87.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. xvi, 383. $17.50.

"Much chaff there is, undoubtedly, but there is also some high-quality grain buried under the mountainous Marxian superstructure imposed by the party. For this accomplishment alone, if for no other, Soviet historiography merits careful attention" (p. xv). Professor Emeritus Mazour of Stanford University has truly given long study and careful attention to his subject. Eschewing, on the one side, "the praise from inside of the party-liners, the adulation of the orthodox Marxists" and, on the other side, "the attacks of its severest critics—the Kremlinologists," Mazour has sought to balance weaknesses and faults against strengths and achievements. His general conclusions, like that quoted above, are cautiously favorable and hopeful. His final words are: "It is truly remarkable how much Soviet historiography has managed to accomplish" (p. 364).

Comparisons between this new book and either the original edition or the 1956 revision of Mazour's *Modern Russian Historiography* are impossible. The earlier books dealt with far fewer historians though they covered a much longer time span. The original volume was both informative and readable: the revision contained more information but was less readable. The present book is crammed with data and will be an indispensable guide and reference though few are likely to choose to read it in its entirety.

*The Writing of History in the Soviet Union* is a labor of love as well as a tremendous tour

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de force. Years of systematic examination and contemplation of works by Soviet historians provided Mazour with far more material than could feasibly be published. He was then faced with the enormously difficult and thankless tasks of excision, elision, and compression. His necessarily arbitrary decisions will certainly be challenged by critics complaining that he gave too much space to some persons and subjects and too little to others. Given the magnitude of the task Mazour set himself, this reviewer is not disposed to join such critics.

Mazour clearly tried to keep the needs and problems of his readers in mind. His index contains roughly 1,600 personal names, interspersed with institutional titles and labels of subjects. The chapter titles and subtitles are descriptive though very brief. Chapter 6, "Socio-Economic Developments, XIX Century," for instance, is subdivided into socioeconomic history, regional agrarian history, growth of Russian towns and cities, the early period, and industrial development. The organization of the material, as this sample indicates, is both thematic and chronological.

One may infer mixed feelings on the part of the unnamed editor who noted that he made no basic alterations in the author's "blend of footnotes and bibliographic entries." The reader will soon discover that the index will usually lead him to both the text and the bibliographical entries. I wish, however, that the publisher had been more alert about occasional repetitions, misspellings, and rhetorical as well as grammatical errors. It is a little disconcerting, for example, to find essentially the same things about the reissue of Kliuchevskii's works in the text on page 33 as had previously been said more fully in text and notes on page 31. A discussion of the problem of periodization of recent Soviet diplomatic history seems curiously out of place amid a lengthier discussion of Soviet publication that includes commentaries on materials from much earlier centuries (p. 39). In spite of such slips, which are not "statistically significant" in a book so crammed with names and titles, Professor Mazour's book will be of great service to scholars.

WARREN B. WALSH
*Syracuse University*

ABRAHAM ROTHBERG. *The Heirs of Stalin: Dissidence and the Soviet Regime, 1953–1970.* Ithaca: Cornell University Press. 1972. Pp. xii, 450. $14.50.

This work is an impassioned but well-documented chronicle of the repression of Russian writers by the Soviet regime since the death of Stalin. It does not go into any detail on the "Democratic Movement" and other political tendencies expressed in *samizdat,* nor does it really analyze three distinct "currents" of dissidence (artistic, political, scientific) as suggested on the jacket and in the table of contents. The focus is on the major writers and the public record of their troubles with the regime since Pasternak's Nobel Prize provoked the authorities in 1958. The bulk of the work is narrowly chronological (aside from the essays on the scientists Medvedev and Sakharov that are rather loosely appended at the end), but useful for putting the record of literary repression in order and letting the facts speak gloomily for themselves.

Abraham Rothberg has combined a journalistic and literary career with a long-standing interest in Soviet and East European affairs. The focus of his attention is Aleksandr Solzhenitsyn, the subject of Rothberg's last book (*Aleksandr Solzhenitsyn: The Major Novels* [1971]) and the central figure in the contest of politics and art recounted in the volume under review. Solzhenitsyn emerges here not merely as a gifted and honest writer but as a pillar of courage and integrity, withstanding incessant harassment to keep alive the principle of artistic freedom. His tribulations, like those of writers less successful in avoiding direct repression, underscore the continuance of the old Russian interweaving of politics and literature, where literature becomes the sole vehicle for political expression and hence invites dictation by the state lest its power be questioned.

Mr. Rothberg's investigation of dissidence does not extend beyond the Russian writers and a few of their scientific friends. The minorities are generally omitted, apart from a passing reference to the Chornovil case in the Ukraine. Popular sentiment is rather cavalierly dismissed as chauvinistic and indifferent to the fate of the intellectuals. Recent evidence of political dissidence within the power structure, such as is suggested in the "political diary," has perhaps appeared too recently to be considered here. Finally, the political context has not

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been fully explored for the motivations of literary repression—for instance, fluctuations in Khrushchev's political health correlating somewhat with the literary climate before his fall and the shift of the successor regime to repression (with the prosecution of Sinyavsky and Daniel) after a year of relative lenience. Mr. Rothberg stresses, however, the impact of the Czech reforms in 1968 on the determination of the Brezhnev-Kosygin leadership to close the door on any dangerous possibilities of artistic autonomy.

ROBERT V. DANIELS
*University of Vermont*

## AFRICA

JOHN S. MBITI. *New Testament Eschatology in an African Background: A Study of the Encounter between New Testament Theology and African Traditional Concepts.* New York: Oxford University Press. 1971. Pp. xi, 216. $7.25.

The study of African religion and religious activity has become a flourishing subfield of African history during the past several years. Africanists in the United States (Ranger, Wright, and others) and Europe have combined with African scholars to take a fresh look at the varieties of religious experience south of the Sahara. In addition, African theologians are raising questions about religion and its significance for developing modern African states. Professor Mbiti's new volume falls within the sphere of both endeavors: it attempts to characterize African traditional religion with examples furnished mainly from the Akamba of Kenya, who he maintains are representative of black Africa. It is also a closely reasoned and documented inquiry into the current dilemma confronting orthodox Christians in Africa, how to make Christianity received from Western missionaries relevant for Africans. In short, how to Africanize the Church.

Mbiti's focus is the encounter of Christian eschatology with African concepts. For instance, he finds the notion of future time (crucial for eschatology) lacking among the Akamba and hence, by extension, in much of traditional society. The Akamba possessed only two time dimensions, past and present, and what Westerners would call the future was really incorporated into the past. This supposedly made it difficult to teach the Akamba eschatological subjects such as the resurrection, last judgment, and end of the world. Missionary attempts to preach about a future Christian world were frustrated because African myths were past rather than future oriented. In the resurrection, for example, the Akamba believe that the dead will rise, but it is not a future event—it occurs only in the *tene* or past period. It is a corporate rather than individual phenomenon (households or families resurrected together); it is brought about by spirits (*aimu*) rather than a central god.

As a practicing professor of Christian theology at Makerere College, Uganda, Professor Mbiti is primarily concerned with how a traditional people's own myths and religiosity condition their perceptions and acceptance of Christianity. But this inquiry also helps explain African perceptions of time and the historical process and makes the study valuable for historians too. An individual's perception of his place in history (and his society's) is a reflection of his understanding of the time dimension. In the context of Mbiti's work this suggests a gap between traditional African conceptions of history (if the Akamba are typical) and Western conceptions, which may explain why some Euro-American field researchers observe "the Africans didn't understand what I was getting at." If Africans have misunderstood Western history, the opposite is certainly true, and therein lies an important contribution from Professor Mbiti.

There is not space to comment on other original theological aspects of this work, such as language in eschatology and corporate resurrection. I regret that Mbiti drew so heavily from Akamba evidence to the detriment of other African peoples. This limited data makes the study suggestive rather than conclusive. But we are in the author's debt for raising important questions for African theologians that also challenge African historians and that both may no longer ignore.

G. WESLEY JOHNSON
*University of California, Santa Barbara*

## UNITED STATES

THOMAS C. COCHRAN. *Business in American Life:*

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*A History*. New York: McGraw-Hill Book Company. 1972. Pp. x, 402. $12.50.

"The hurdy-gurdy, the birdie, the man on the street,/The candle-maker, the baker, the cop on the beat,/The village charmer, the farmer, the man in the moon/All sing Elmer's tune."

Elmer is a businessman, says Professor Cochran, and American society dances as well as sings to his tune. In each of the four classic periods of our political and social history business organization and behavior have developed in response to wide, but changing, economic opportunities. A heritage is carried over from the immediate past, but this is reshaped to permit exploitation of the present. This reshaping has modified business forms and the dominant personality types within them. It has affected also law, politics, education, religion, and the family, including especially the practices and precepts of child-rearing. This malleability of American society and personality has given continued dominance to a business ethic and standards of achievement and has enabled the economic side of society to develop, giant-like, dwarfing all other sides of life and all other institutions and value systems and forms of behavior. It is recognized, of course, that a culture with a strong business component is not peculiar to the United States. But special conditions—notably a migrant and migratory population, abundant resources, and growth in the heyday of market capitalism—made business dominant in American culture and society as in none other in recorded history.

It is fascinating to follow Professor Cochran as he makes his case. A lifetime in American business history has furnished him the historical learning. A genial tolerance of the social sciences has furnished the introductory generalizations and some of the vocabulary. A perfect reasonableness and gentle authority has furnished the tone—*sine ira et studio*. Each of the four sections—colonial, antebellum, postbellum, modern—takes up first the problems and opportunities for business, then the adaptations in forms and the modal business personality; then the discussion proceeds on to neat chapters on culture, law, politics, and social structure. The writing is sober but interesting, and by a kind of straight-faced mildness—even blandness—dissent is quieted and we are led, or taken, in. In business this is called the soft sell. In scholarship it is simply the reasonable, straightforward statement of a theme by a sensitive historian who has given his life to following it. The art is so great and so natural that one never is allowed to realize what labor and learning it contains, or how strong, methodologically and substantively, are its implications.

There are, of course, problems with so straightforward a presentation of a complex subject. On the substantive level Cochran produces a rather muted history, robbed both of strong structure and the excitement of uncertainty. The structure that Sombartian economic history gave to American economic history had a beginning, a middle, and possibly an end: early capitalism in colonial mercantile ventures, high capitalism in competitive small and medium-scale manufactures of the mid-nineteenth century, and late capitalism in the age of big business. And possibly since 1930 there has come a period that the German economic historians would hardly recognize as capitalism at all. Such a dramatic structure is not here wholly absent, but its climax in the period 1850–90, where Professor Cochran has done such interesting earlier research, is muffled. In place of the hero-villain, Capitalism, with its perilous and dramatic life cycle, we have a role-player business, whose victory is never in doubt, a kind of Holy Ghost of American culture, benign, intangible, and omnipresent. What would have been required to make or keep America a slave society, or an agrarian democracy, or a paradise of little entrepreneurs, or a workingman's state, or an empty wilderness—another Brazil or Canada? It may seem frivolous to inquire, but history without a counterfactual hypothesis behind the scenes is in danger of being otiose. Professor Cochran in proving his point has nearly smothered it.

On the level of historical method, there is a similar muting of antagonisms. The social sciences, after all, are not truth, available to anyone endowed with reasonable common sense. They are logical structures within which human minds operate. We call them "disciplines," and this means that they to some degree exclude one another. Economics—which is a bit missing from Professor Cochran's book, despite his known familiarity with its major emphases—explains one set of historical data, employing a notorious array of *ceteris paribus*

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conditions about social structure, knowledge, personality, and politics. Sociology, social psychology, political science explain change in these other streams of historical phenomena with the limits of assumed models of human behavior. In history, one often feels, we are looking for a means to develop "feedbacks" from these streams of change, one into another, so as to see as a comprehensible system an even larger portion of moving historical reality. Professor Cochran knows this, one feels, but one wishes he had more formally and forcefully articulated it. Still he has produced a decidedly original book, less superficially exciting than most new syntheses that appear, but perhaps more trustworthy and believable for its moderate tone and its evidence of its author's wide learning and sympathies.

WILLIAM N. PARKER
*Yale University*

IVOR NOËL HUME. *A Guide to Artifacts of Colonial America.* New York: Alfred A. Knopf. 1970. Pp. xviii, 323, vi. $10.00.

Ivor Noël Hume has written an authoritative compendium of information about objects that inhabitants of the English colonies in America used in the operations of households. Historical archeologists, antique collectors, museum specialists, and social historians will all find it valuable. As historians begin to ask questions about human behavior and its development in various social contexts, they are coming to appreciate the testimony to be found in shards of pottery or fragments of glass from tightly dated archeological sites. What were the habits and tastes of our forefathers and how quickly did these change in response to new technologies or styles? Did adaptability or resistance hinge on position in the economy, social station, or the frequency of exposure to new experiences? Documents alone will not answer such questions, especially in tracing the history of people who could not, or did not, commit their thoughts and feelings to paper.

Like Mr. Hume's earlier books, his introductory essay supplies an eloquent as well as witty plea for attention to the "sign posts of the past" that are supplied by properly identified archeological finds. The discussion of the sign-posts themselves is organized alphabetically, beginning with armor and ending with wig curlers. The information is based on finds taken from sites accurately dated, so that changes in technology and taste can be followed through time and from place to place. The social historian who uses estate inventories to study households will find answers to many questions, although this book does not pretend to be an encyclopedia that will identify every mysterious item. The historian willing to learn from artifacts themselves in relation to their time and place will find this book indispensable.

Both historians and historical archeologists suffer major difficulties in studying the life of the very poor. Collections of well-researched artifacts are likely to come from middle- or upper-class house sites, and poor householders are less likely than the rich to be inventoried. The evidence about the poor is often negative, relying on what does not appear in inventories or trash pits. Hence this book necessarily is more useful for studying middle- or upper-class styles of life than the life of people on the bottom. Greater collaboration of historians and archeologists in asking their questions and in sifting their evidence would enable both to contribute more to our knowledge of all elements of a society and of the changes that take place over time. In making available so much of the information already gathered about the artifacts of colonial America, perhaps this book will encourage such collaboration.

LOIS GREEN CARR
*St. Mary's City Commission*

WESLEY FRANK CRAVEN. *White, Red, and Black: The Seventeenth-Century Virginian.* (Richard Lectures for 1970–71, University of Virginia.) Charlottesville: University Press of Virginia. 1971. Pp. vi, 114. $5.75.

The James W. Richard Lectureship at the University of Virginia, from which C. Vann Woodward fashioned *The Strange Career of Jim Crow* (1955), has again led to a concise and suggestive volume by a distinguished historian of the American South. Wesley Frank Craven has written three brief but substantive essays on Native Americans, Africans, and Europeans in the Virginia colony. His lectures are valuable when taken separately, and invaluable when juxtaposed in a single book. It is a significant conceptual step when a volume subtitled

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"The Seventeenth-Century Virginian" can give serious consideration to all the people living within that chronological and geographic boundary.

Asking questions prompted by demographic historians, Craven begins with an essay that maps out the dimensions of the migration of whites to seventeenth-century Virginia. He uses an extensive series of land-grant abstracts to estimate the size of this much-studied movement and to analyze fluctuations in the migration over time. Craven demonstrates that more than half the Englishmen (mostly West countrymen) who sailed to Virginia before 1700 embarked during the quarter century after 1650, and he suggests that such a concentration may be explained as much by the series of English crop failures during these years as by political, military, or religious events. Immigration declined markedly during the last quarter of the century, and by 1700 a majority of Virginia's whites had probably been born in America rather than Europe. More strikingly, the number of Virginians with English ancestry living in 1700 may not have equalled the number that had migrated during the entire preceding century, a fact that sharply differentiates the Southern settlement from the more rapidly growing colony in Massachusetts. Craven accepts the plausible explanation that a higher ratio of men to women existed among Virginia's white immigrants than among Puritan settlers. But he may underestimate the degree to which disease was a separate factor slowing European population growth in the tidewater area.

The second lecture underscores another difference separating Massachusetts from Virginia: the contrast between the straggling Wampanoags and the powerful Powhatans. Chief Powhatan's confederacy of half a dozen Algonkian-speaking tribes numbered eight or nine thousand when the English arrived, and Craven outlines the effects of foreign intrusion upon the "relatively sophisticated economy" of these agricultural people whose land became Virginia. In doing so, he points the way toward, though by no means exhausts, relevant anthropological data of the sort too long ignored by historians.

By comparison, Craven's sources for discussing black Virginians are traditional, but he demonstrates how scholars preoccupied over Negro "status" have long ignored the most basic demographic questions and obvious social facts. At one point he even provides insight on the famous first arrival of "twenty Negars" in 1619. Craven's entire book is a testimony to the interrelatedness of red, white, and black histories in the colonial southeast, and it offers an implied challenge to authors and teachers approaching other segments of American history.

PETER H. WOOD
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JOHN W. REPS. *Tidewater Towns: City Planning in Colonial Virginia and Maryland.* (Williamsburg Architectural Studies.) Williamsburg: Colonial Williamsburg Foundation; distrib. by University Press of Virginia, Charlottesville. 1972. Pp. xii, 345. $15.00.

With *Tidewater Towns* city planning historian John Reps inaugurates a series of regional histories that will amplify and extend his general survey, *The Making of Urban America* (1965). In both he is intent on showing that planned cities are as much rooted in America's apple-pie tradition as subdivisions and trailer courts. The first book pushed west out of the tidewater region after founding Annapolis and Williamsburg. This present volume goes back and, by including expanded or entirely new chapters on the new town acts of seventeenth-century Virginia and Maryland, the independent creation of eighteenth-century communities, and the rapid growth of Washington, Richmond, and Baltimore, suggests to those who are concerned about modern urbanization that direct action by governments in the planning process is neither un-American nor ineffective.

Reps asks how towns looked rather than how they worked. The answers he sometimes finds in economic or legislative history, thereby giving this book more sophisticated explanations than the brevity of the survey allowed. Nevertheless his approach to research and writing is fundamentally an art historian's. Traditionally they start with an artifact (here an American town plan), then ransack the past for prototypes (European town plans), and conclude by finding links—books usually—by which the one might have influenced the other. This kind of detective work employs skills the ordinary AHA historian lacks: an ability to find infor-

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mation in physical things, for instance, and a willingness to use such unconventional sources as archeology. Reps's special talents here make *Tidewater Towns* a better book.

But the rhetoric of art history has limits. Appearances alone can deceive, as they do when Reps looks for precedents for America's earliest town plans. Although Elizabethan economic geographers occasionally dreamed of sending "infinite nombers" to settle America as Spain had, they proposed instead a coastal system of fortified commercial outposts staffed by company agents sent out to organize trade with the Indians. The towns—Sagadahoc and Jamestown, for example—were designed to serve a very specialized purpose. What was the source of their distinctive plan? Reps, his art-historical eye fixed on form, suggests that Edward I's bastide fortresses are "not improbable" or English towns with Roman origins "not too unlikely" or colonial settlements in Ulster "far from inconceivable." Timely publications by William Camden and John Speed prove all was possible. But another approach supplies a liklier explanation. Researchers who look for parallels for the function of these trading stations will discover Portuguese West Africa, where a colonial system identical with the one proposed for America was run from "trade castles" much like the first New World forts. Frequent references in England's state papers show that officials were collecting information about Portugal's Guinea trade at the same time that planners like the Hakluyt cousins were formulating colonial theory.

The history of American city planning needs investigation from different viewpoints. Reps has started an ambitious serial study that will interpret the physical evidence for the social and economic historians who, one hopes, will follow.

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DAVID H. FLAHERTY. *Privacy in Colonial New England*. Charlottesville: University Press of Virginia. 1972. Pp. xii, 287. $12.50.

In recent years social historians of colonial New England have begun to analyze systematically the social behavior and experience of ordinary people. This useful study advances this new concern through the examination of privacy, defined to include solitude, intimacy, anonymity, and reserve. The difficulty and challenge involved in writing a history of privacy should be obvious. With the exception of rare introspection in diaries, written records are generated by social interaction. Hence historical evidence more often relates to the violation of privacy rather than to its existence.

Unlike most of the recent social historians of early modern society, Flaherty is unimpressed by the sense of radical differentness of the "world we have lost," to use Peter Laslett's charming phrase. Based on the assertions that "human nature is fundamentally the same today as in the early days of the nation" and that the desire for privacy is characteristic of "human nature from time immemorial," his responses to "Did privacy have any meaning for past ages? Or was concern for privacy a product of the unique needs of twentieth-century society?" seem unarguable. Stating the issues in this fashion, however, renders the central conclusion both true and trivial. Fortunately the rhetorical questions are not allowed to cripple his wide-ranging, well-researched, and traditional historical analysis of the social contexts of privacy in colonial New England. The historical importance of privacy as a value hinges on priorities assigned to competing values. The significance of privacy as a historical fact depends on the seriousness and incidence of its violation. Flaherty is keenly aware, for example, that privacy was constrained by Puritan watchfulness, by a legal tradition of moral authoritianism, by large families who often shared dwellings, and by informal pressures inherent in the small communities where most of the population lived. If privacy was limited by alternative values such as sociability and ecological factors such as family size, so too, he forcefully argues, were the organized attempts to deny the enjoyment of privacy. Presented in detail and with scholarly integrity, the evidence permits the reader to make alternative conclusions. Although social historians will be convinced, for example, that unmarried colonial lovers undoubtedly sought and usually achieved privacy for sexual intercourse, they may question whether colonial fornicators were discovered less frequently than at the

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minimal rate of 6 per 100,000 coital acts reported by the twentieth-century American women studied by Kinsey.

Still, the insistence on an American tradition of privacy limits the value of this study for historians. While he documents the waning of the negative influence of Puritanism on privacy in several areas, his dichotomous framework forecloses the possibility of a sustained analysis of the decline of social control during the late seventeenth and eighteenth centuries. Instead, colonial New England is treated as a coherent unit that is contrasted to modern America. Nonetheless, the book is an important contribution to the ongoing dialectical debate on society and personality in early America.

DANIEL SCOTT SMITH
*University of Connecticut*

WILLIAM G. MCLOUGHLIN. *New England Dissent, 1630–1833: The Baptists and the Separation of Church and State.* In two volumes. (Publication of the Center for the Study of the History of Liberty in America, Harvard University.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xxiii, 693; vii, 697–1324. $35.00 the set.

No other region of the United States has been studied in such careful detail or with such sophistication as New England. Scholarship directed toward unraveling the complexities of the region's religious history has been of particular merit. In most of this work, however, the history of the New England Baptists has been either omitted, oversimplified, or mentioned in a half-hearted attempt at completeness. This has not always been unjustified for the Baptists were never dominant in New England. Nevertheless the history of the Baptists, a dissenting minority, is important for what it reveals about the larger society. Because they were outsiders the Baptists had a critical perspective from which to view "the New England way," and the interrelationship between the upstart Baptists and their committed competitors makes for a compelling drama. There is much more to the Puritan experience—and New England—than what has been told by the Puritans and their ancestors. The Baptists fought long and hard for their religious freedom (though not necessarily for the freedom of others). Even though they are not unblemished heroes, their arduous struggle for social and political equality depicts with startling clarity the repressive underside of the New England establishment.

The story of the Baptists has finally begun to appear in careful academic scholarship. Ten years ago C. C. Goen published a pioneer study that richly presaged what lay in store. His capable work was later complemented by McLoughlin's perceptive biography of Isaac Backus and by a bevy of articles. But not until the publication of McLoughlin's monumental *New England Dissent* has the story been told with a completeness and analytical subtlety to match the corpus of Puritan historiography. Despite some minor faults, McLoughlin's magnum opus has redeemed the Baptist contribution for the foreseeable future. No one will ever be able to see the history of the first two centuries of New England in quite the same way again. That is the true measure of McLoughlin's achievement—he has inalterably reshaped a significant portion of our historical landscape.

As with most mature scholarship, McLoughlin's work has not effected this change by bulldozing aside previous interpretations. Those familiar with his earlier writing and that of Goen, Alan Heimert, S. E. Mead, E. S. Morgan, and several others, will find many recognizable themes. Building on their work and on his own mountain of research—not confined to comfortable printed sources as with too many intellectual historians—McLoughlin has added a simply unrivaled depth of context and understanding to a conceptual framework that was previously sketched. The elusive intricacies of the Puritan/Congregationalist and Baptist varieties of pietism; the combination of idealism and practicality with which the Baptists carried on their struggle; the complex ways in which local circumstance determined both the substance of the debate for voluntarism and the outcome; the tangled arguments in courts and pamphlets; the cautious participation of the Baptists in politics; the relationship of Jeffersonian Republicanism to the theological and ecclesiastical disputes; the activities of Anglicans, Quakers, and Universalists in behalf of disestablishment; the Baptist efforts to gain respectability—these and numerous other topics are treated in meticulous detail. For each topic McLoughlin recreates the complicated welter of personality, ideas, and method that

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shaped what happened. He never hesitates to place blame or give praise; the contradictions and defeats of the Baptists are told as honestly as their triumphs. Their basic conservative stance on the role of religion in society is made clear; ironically on this issue they were often very close to the establishmentarian position. Again and again McLoughlin's control of detail gives sharp focus to what until now has been vague generalization. The validity and limits of each point are remarkably well developed, providing a healthy corrective to recent studies of religion and the American mind.

Encyclopedic as this work is, it nevertheless sustains an interpretive structure. Four motifs govern the 1,282 pages of text: "the emergence of voluntarism" (p. xviii); the Baptist search for an equal position in New England society; "the theme of persistent pietism" (p. xix), which McLoughlin argues is central to an understanding of the American character; and the Baptist progress as a mirror of "the transformation of New England from a conformist, elitist, corporate Christian Commonwealth to an atomistic, individualistic, laissez faire, quasi-secular democracy" (p. xix). In McLoughlin's hands these four controlling themes are deftly used to give broader meaning to a historical experience that might otherwise have been confined to denominational historians. He has written a magisterial study few American historians can afford to overlook.

It is precisely the great significance of the work that leaves one with several regrets. Perhaps McLoughlin's single most persistent theme is his emphasis on the pietistic tradition in America, and under this concept he subsumes such a staggering diversity of positions that the term finally becomes vacuous. Although he shows that sincere religious beliefs were the ultimate motive force in the drama he unveils, he never makes clear why some people could be so overwhelmed with such conviction and others left practically untouched. This is probably an unanswerable question, but it is nagging. Unfortunately the length of the books will discourage any but the most determined readers. Even for them McLoughlin's major themes are sometimes lost amid the detail. He has too much of significance to say to let his format limit his message. One can hope that McLoughlin will now produce a much abridged, more closely interpretive work that will appeal to a wider audience. A generation of scholars would be the beneficiaries.

JOHN B. BOLES
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JAMES T. LEMON. *The Best Poor Man's Country: A Geographical Study of Early Southeastern Pennsylvania.* Baltimore: Johns Hopkins Press. 1972. Pp. xviii, 295. $12.00.

This book deserves careful attention—if only as an intensive local study that does not examine New England or the Chesapeake, the regions that heavily dominate the scholarship of colonial America. Most broad interpretations of the United States before the Civil War assume that Massachusetts or Virginia provided the nation with both its social structure and its ideology. For Perry Miller, America became Puritan New England writ large, but most students of political behavior define the nation in political terms and implicitly locate its norms in Jeffersonian Virginia. Perhaps most American ideals can be traced to one or the other, but the hard social realities of the nineteenth-century republic, James T. Lemon insists, derived from neither. The dilution of community, high geographical mobility, the autonomous farm, the dominance of the profit motive, the weakening of government at all levels, religious diversity, and ethnic pluralism without significant emphasis upon slavery—all of these features originated in their purest form in the middle colonies and then spread to the Ohio Valley, the Middle West, and the prairie states. Lemon calls this reality "liberal," a normative term that he tries to render descriptive when he asserts its primacy for later American development. But for the most part he avoids extensive speculation on this theme in favor of a minute dissection of eighteenth-century Pennsylvania.

In this study the conventional boundaries of American academic departments almost completely disappear. Lemon is a professional geographer, but historians will read his book as an imaginative approach to social history, a field that has become heavily interdisciplinary in recent years. He demonstrates that geography, quite as much as demography, child psychology, or the sociology of the family, can organize and interpret data that has remained in-

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tractable to more conventional methodologies. The ninety-three tables and charts that crown Lemon's pages might tire more than a few readers, but they certainly testify to his thoroughness and often to his ingenuity. Both qualities were essential to his purpose because the "local" region he has studied is much larger and far more populous than the areas Philip Greven and Kenneth Lockridge have examined in New England. His investigation covers about two-thirds of the settled portion of colonial Pennsylvania, with special emphasis upon Chester and Lancaster Counties.

Lemon patiently analyzes environment, soil types, the Old World origins of particular groups, and other variables as well. Few stereotypes, whether cultural, ethnic, or geographical, survive this scrutiny. He flatly denies that Germans were better farmers than English or Scots-Irish settlers, and he frequently notes that the Scots-Irish had to share the mid-eighteenth-century frontier with Germans. On the other hand, religious differences did matter. "Sect" people prospered more than "church" people of the same ethnic background even when they had less favorable soil to work. Yet farming everywhere remained extensive rather than intensive, and the advice of agricultural reformers went unheeded because much of it was imported unthinkingly from England and was not particularly relevant to local conditions in Pennsylvania.

Lemon's other findings seem more predictable in terms of existing scholarship. As elsewhere in the colonies, the gap between rich and poor widened over the century and partible inheritance became less common over time. Between 1760 and 1782, about thirty per cent of Lancaster County's married taxpayers were landless and another thirty per cent were tenants, some probably sharecroppers. Horizontal mobility, though difficult to measure with precision, was much higher in Pennsylvania than in New England. Most farmers were market-oriented, rather than community centered, an emphasis that created a belt of secondary cities in the interior after 1730. "Quite possibly for the time," he concludes (p. 227), "more [Pennsylvanians] lived better than any other population of similar size in the world."

Political historians might quarrel with some of Lemon's judgments. "Social unity contributed to a prosperous society," he declares (p. 23) while trying to deflate the significance of ethnic antagonisms. But we never learn what this "unity" was, and it certainly had difficulty achieving political expression. Gary Nash's dissection of political behavior into the 1720s indicates that Pennsylvanians could not even agree on the most rudimentary principles of how—or whether—they ought to be governed. The polemical literature of the 1750s and 1760s suggests that Lemon's colonists possessed extraordinary talents for ethnic vilification. If in this respect Lemon somewhat overdoes his assault upon stereotypes, he fails to devote comparable analytical energy to his own stereotypical concept of "liberal." For example, why did community-oriented sectarians profit more than their individualistic and materialistic competitors? And why did the less successful church people, and the unchurched, triumph over the sects to produce a "liberal" environment that highly valued acquisitiveness? The use of such categories might stimulate more confusion than it resolves.

But these faults are minor. *The Best Poor Man's Country* is a distinguished and important book, a fitting addition to the recent Chesapeake studies of Aubrey Land and the New England efforts of Greven, Lockridge, John Demos, and Sumner Chilton Powell.

JOHN M. MURRIN
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JAMES H. HUTSON. *Pennsylvania Politics, 1746–1770: The Movement for Royal Government and Its Consequences.* Princeton: Princeton University Press. 1972. Pp. viii, 264. $8.50.

The subtitle of this monograph is more descriptive of its contents than its title. Hutson has constructed by far the fullest and most perceptive account to date of the attempts by Franklin and the Quaker party to replace proprietary with royal government in the mid-1760s. Franklin emerges as less politically astute than formerly supposed for he overestimated his ability as the Assembly's agent in London to maneuver the request for royal government through the serpentine waters of ministerial politics and he profoundly misjudged public opinion in Pennsylvania, which regarded the cure of royal government as worse than the disease of proprietary rule.

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But as a study of Pennsylvania politics between 1746 and 1770 the book is less satisfactory. We are at the point in historical studies when political history is expected to be reconstructed from more than the letters and records of upper-class leaders without reference to social forces silently at work beneath the surface of events. Hutson, however, remains in the older mode of analysis. Thus one gains no sense of changing social and economic conditions in the quarter century under review. It is of only minor importance to the analysis that during this period Pennsylvania experienced massive immigration of Germans and Scots-Irish; that the population of Philadelphia, the nerve center of politics, doubled; that wealth and investment capital became far more concentrated; and that disease and poverty became widespread in Philadelphia.

This unwillingness to examine less accessible sources of information leads to oversimplified and questionable hypotheses. For example, Hutson portrays an unchallenged Quaker control of politics in the late 1740s and early 1750s, with the proprietary party serving as "a centerfold advertisement for consensus historians." Though it is true that for about a decade the proprietary party was relatively inactive at the polls, it worked throughout this period to recruit German support and exerted great, ever-growing influence through the council, the courts, and the municipal government in Philadelphia.

The analysis is even less satisfactory in the latter stages of the book. Hutson makes a real contribution in analyzing the realignment of political parties after 1764 when the "popular" Quaker party, bent on calling in royal government, gave up its opportunity to lead the resistance to imperial reorganization and by 1770 had lost most of its artisan support in Philadelphia. But he makes no attempt to look at changing economic and social conditions in the city and hinterlands in order to understand the emergence of the "radical" or "Presbyterian" party. Thus the Philadelphia merchants' refusal to support the non-importation movement in 1767 is explained by the "politics of ingratiation"—the tattered hope that if the colony acquiesced to the new British policies, Franklin could still push through the proposal for royal government. Little is said about business fluctuations that had wracked the mercantile community from 1760 to 1765 and the uses to which both importation and nonimportation could be put depending upon market conditions and inventory levels. Similarly it is rightly pointed out that city artisans had been avidly recruited by both parties in the campaign for royal government and by 1768 were regarded by the emerging Presbyterian party as indispensable. But in viewing political history from the top down it is impossible for Hutson to understand the economic basis for artisan shifts of political allegiance or to make clear the degree to which they were not simply recruited, manipulated, and controlled by party leaders but began to assume leadership and to articulate an ideology of their own in the last years before the Revolution. Thus the book may be regarded as the last word on the movement for royal government, but it is far from complete as a study of mid-eighteenth-century and pre-Revolutionary politics.

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S. K. STEVENS *et al.*, editors. *The Papers of Henry Bouquet.* Volume 1, *December 11, 1755–May 31, 1758.* Harrisburg: Pennsylvania Historical and Museum Commission. 1972. Pp. xliii, 421. $12.00 postpaid.

Henry Bouquet was a Swiss soldier of fortune recruited into British service early in the Seven Years' War, when a desperate shortage of military manpower in England led to the creation of a new regiment, filled heavily with German-speaking soldiers, led partly by officers like Bouquet who had recently been in Dutch service, and named the Royal Americans. Bouquet came to America with the regiment in 1756; for the next nine years, until his death from yellow fever at Pensacola in 1765, he played an important role in Anglo-American military operations and civil-military relations. The high points of his career occurred in Pennsylvania: at the fall of Fort Duquesne in 1758 and in the suppression of the Indian uprising of 1763. Accordingly, for more than thirty years the Pennsylvania Historical and Museum Commission has actively forwarded the publication of the Bouquet papers, most of which are in the

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British Museum. The present volume is the latest product of their continuing efforts.

Between 1940 and 1943 nineteen volumes of mimeographed transcripts, or something more than half the Bouquet collection in the British Museum, appeared as a WPA project. The end of the WPA stopped this valuable work, but after the Second World War it was decided to prepare a letterpress edition that, unlike the WPA transcripts, would rearrange the British Museum bundles into a more usable chronological series. In 1951 Volume 2, covering the 1758 campaign against Fort Duquesne, appeared; the editors then expressed their hope that its publication might uncover new material on the sparsely documented earlier years of Bouquet's career. Volume 1 has at last been published, but two decades have done little to turn up documents that were not already known in 1951. Drawing mainly on their own work as editors of the WPA volumes Dr. Stevens and Mr. Kent have also added a number of important letters and memoranda from the Loudoun papers in the Huntington Library, as well as a few items from other collections, and they have provided the letterpress version with helpful annotation missing in the WPA series.

Difficulties over quartering and recruitment in 1756 and 1757 and planning and preparation for the campaign of 1758 are the major subjects of this volume. Only a brief but valuable sketch, first published in 1952 and based on the few extant Swiss, Italian, and Dutch records, fills in the first thirty-six years of Bouquet's life. For students of the Great War for Empire there are no surprises; for other historians, the routine correspondence of military administration and logistics may seem dull, enlivened occasionally by Americans defying royal authority in Philadelphia, Charleston, and on the Pennsylvania frontier and by Bouquet being hustled out of his command in South Carolina, ostensibly because he had invested a little too deeply in Carolina lands, but more likely because he was outmanuevering Governor Lyttleton in the jungle of Carolina politics. American behavior exasperated Bouquet, but American opportunities attracted him strongly. He complained often about the weakness of colonial government, but volume 1 ends with his learning how to get things done in America without invoking the wrath of Westminster. Present plans call for completing the series in five more volumes that will cover the years from 1759 to 1765.

JOHN SHY
*University of Michigan*

MARSHALL SMELSER. *The Winning of Independence.* (The Quadrangle Bicentennial History of the American Revolution.) Chicago: Quadrangle Books. 1972. Pp. 427. $10.00.

DON HIGGINBOTHAM. *The War of American Independence: Military Attitudes, Policies, and Practice, 1763–1789.* New York: Macmillan Company. 1971. Pp. xvi, 509. $12.95.

IRA D. GRUBER. *The Howe Brothers and the American Revolution.* New York: Atheneum, for the Institute of Early American History and Culture. 1972. Pp. ix, 396. $14.95.

The approaching bicentennial has already started an outpouring of books on the American Revolution. The historian is bound to welcome them because they make clear—as the American Revolution Bicentennial Commission, apparently operating out of Disneyland, does not—that we are celebrating a series of events in history. The outpouring also indicates a host of would-be readers, at least in the publishers' opinion, which is good news these days in any historical field. But such considerations, however cheering to the profession, matter less to it than quality, which depends upon the new data, interpretations, or synthesis that any particular author brings to the retelling of a familiar story. Judged by that criterion the three volumes under review vary widely. The first two, *The Winning of Independence* and *The War of American Independence,* are what their titles suggest, histories of the entire conflict; they are based for the most part on secondary material and, although they do justice to both sides, inevitably emphasize the American. The third, *The Howe Brothers and the American Revolution,* covers a more limited period, relies heavily upon manuscript sources, and is focused on the failure of the two Howes to exploit, in 1776–77, their country's best chance of victory. All three books deal expertly with fundamental issues of the war, but there the resemblance ends. As the authors' aims are different, so is the scholarly value of their contributions.

*The Winning of Independence* is the first in

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a projected six-volume history, edited by Leonard Levy, of the evolution of the colonies into a republic, 1763–87. Professor Smeltser is writing for the lay public; he does not expect to, and does not, surprise or instruct the scholar. But his style is vivid (the war "proceeded like sporadic fireworks in a blackened sky"), his narrative is skillfully interwoven, and his interpretation is shrewd if orthodox. He brings out the salient aspects of the war, civil as well as military, and makes clear that it ramified on the American side, just as it did on the British, into internecine conflicts in the Congress and the states; his final summation of why the struggle ended as it did is a model of objective and concise good judgment. This is not a book that will set the academic Thames on fire, nor is it intended to be; for the public at which it is directed it leaves little to be desired.

*The War of American Independence,* the sixth volume to appear in the series edited by Louis Morton on the wars of the United States, is broader in its time span than Smeltser's book and, as its subtitle indicates, narrower in its focus. The treatment, however, is anything but narrow: "attitudes, policies, and practice" turn out to be a rubric that covers matters as diverse as economics and technology, the press and pulpit, the traditions that determined how the war was fought on both sides, and its impact in Europe and America. This is not military history as it often is, the arid detailing of campaigns, but as it ought to be, the study of complex and disparate factors coming together to set the course and outcome of a war. Some of the chapters seem at first like digressions that interrupt the story, but Professor Higginbotham wisely subordinates narrative continuity to the large-scale synthesis that is the particular virtue of his book. His focus is an armed conflict; his subject is a whole society, and he has something to say to any historian of the period.

In discussing the first phase of the war both Smeltser and Higginbotham acknowledge, the second more cautiously than the first, their debt to what may be called the Gruber thesis. The Howe brothers were so determined to settle the quarrel by negotiation, according to Professor Gruber, that they subverted their government's intention to settle it by force. The thesis has been suggested before, but never elaborated in a work of such impressive scholarship. *The Howe Brothers and the American Revolution* differs from Smeltser's and Higginbotham's volumes in two respects; neither difference makes it a better or more valuable work than theirs, but both entitle it to fuller consideration here. In the first place it is quarried out of the sources, and in the second place the author draws from them a conclusion, controversial but not to be dismissed lightly, that goes to the root of the question of why the British failed.

Gruber has covered not only the mountain of primary printed material but also an amazing range of manuscripts. He seems to have overlooked nothing (except the diary of that embittered gossip, William Smith), and has uncovered a surprising amount of new material. Although most of it is peripheral to his main theme, its discovery in a field already so well explored is a remarkable achievement. The originality of his research, if I may carp, is unnecessarily magnified by his tendency to cite manuscripts in the archives rather than in printed form, and to ignore other authors who have used them, and sometimes even the same quotations from them; his footnotes consequently suggest that he is on fresher ground than he usually is. But this is a minor and doubtless unintended shortcoming in a work that stands on its own solid base.

Is the base solid enough to support the thesis? Does the evidence adduced, massive as it is, justify even a strong presumption that the Howes spurned their military opportunities, as Gruber concludes in his final sentence, because they were seeking peace? This conclusion is tenable only after two prior points have been established: that both brothers, not one only, ignored major opportunities, and that they did so in order to bring about reconciliation. The record leaves both points in question. Although Sir William undoubtedly failed, time and again, to exploit his chances, his brother had precious few to exploit; the admiral's one campaign at sea, in 1778, was inconclusive but revealed in him a mixture of caution and driving initiative. To say that Sir William's initiative was evanescent, furthermore, is not to say that even he spurned his chances; he may have let them slip out of strategic myopia or indecisiveness or fear of casualties, or he may have

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been a bear of small brain. He certainly had a genius for throwing away the fruits of success, but his motives, if he had any, remain obscure. As for his brother, he gathered no fruits.

As the book progresses, the author develops his thesis so subtly that it is difficult to examine. A few examples will illustrate the technique. The Howes obtained their command "by obscuring their own intentions" (p. 71). When they landed on Staten Island Sir William "probably" decided to avoid a decisive action, and Lord Howe "would have favored—and may have helped bring about—such a change in his brother's plans" (p. 107). After their hopes of conciliation faded, they "reluctantly" planned to drive the rebels from Manhattan (p. 109). The admiral not only "was anxious that the war be restricted as much as possible and probably helped persuade his brother to modify his strategy accordingly" (p. 126), but also believed that the only way to promote a lasting reconciliation was "by keeping his command and deviating from his instructions" (p. 143). None of these passages is documented; each is a more or less hidden persuader. "Perhaps" the Howes concealed their intentions in order to obtain command; or perhaps, being human, they had no clear-cut idea of what faced them across the Atlantic. If the admiral induced his brother to avoid decisive action in the New York campaign, why did the general adopt Clinton's plan for the Battle of Long Island? Who knows whether the brothers were reluctant, or merely misguided, in the moves they made after that battle? "Black Dick" did keep his command and deviate from his instructions, and he was interested in reconciliation; but the interest had no necessary connection with the deviation, which was common among admirals who never troubled their heads about politics. Gruber fits the Howes' conduct, in short, into a framework of motive that is ingenious and provocative, possibly true, but still supposititious. He does not have the evidence to prove his case.

This may be regretted, because we are hot for certainties; yet the Howes cry out to be explained, or at least Sir William does, and the effort to find an explanation is worth while even if only conjecture results. The two brothers loomed so large in a crucial time that they merit even more attention than they have had, and Gruber has finally given them their due in a work of major importance. It supersedes that of Troyer Anderson a generation ago, partly because it is based on much broader research and partly because it elucidates, as Anderson does not, the complex personal interplay between Whitehall and its commanders. The Howes had strong roots in the establishment, as strong as Cornwallis's, and Sandwich and Germain had to walk warily in dealing with them and even more warily in getting rid of them. If the admiral did not carry out the policy that the first lord expected him to, the general was an even greater disappointment to the American secretary. With reason. No one except Washington, probably, did as much as Sir William to ensure the survival of the United States until the French alliance: where the American lost campaigns and escaped defeat, the Englishman won them and escaped victory. He may possibly have tried to escape it because he considered it the wrong goal; he more probably sought it by the wrong means. But in any case his role, and to a lesser extent his brother's, were so pivotal that this study of them will long deserve the attention of scholars.

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*Yale University*

BARBARA GRAYMONT. *The Iroquois in the American Revolution.* (New York State Study.) [Syracuse:] Syracuse University Press. 1972. Pp. x, 359. $11.50.

"This is a family quarrel between us and Old England," the Continental Congress assured the Iroquois in August 1775. "You Indians are not concerned in it." But by May 1776 the Americans were trying as hard as the British to secure Indian allies for their own side of the "family quarrel" that was to splinter the League of Six Nations and deprive them of their homeland.

Barbara Graymont has told the story of *The Iroquois in the American Revolution* with competence and impartiality. From a critical acquaintance with a wide range of printed and manuscript sources she has assessed the responsibility on both sides for dishonoring the Iroquois pledge of neutrality and leading them into a conflict from which they could only emerge the losers. Her criticism of the savage Sullivan-Clinton expedition of 1779 is espe-

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cially welcome. Although the army destroyed forty Indian villages and the agricultural heart of Iroquoia, "the power of the Iroquois was never broken until the end of the war when the British abandoned the Indians and turned their country over to their enemies, the Americans."

The weaknesses of this solid book are few and mostly minor. Its major drawback is a somewhat narrow focus. The portentous history of Indian-white relations in the Northeast, including the Albany Congress of 1754, is neglected as is the role of the New York Indian campaigns in the larger theater of war in the colonies. There is too little discussion of the actual military significance of the Iroquois beyond their numbers; Indian technology and especially tactics made a greater contribution to American victory than the author reveals. And Sir William Johnson's influence with the Indians is not sufficiently explained by references to his marriages and casual liaisons with Indian women. Even Mary Brant's stature among her people before her husband's death in 1774 is not clarified.

The hero of the story is the Mohawk Joseph Brant, but the source of his authority is left unclear by confusing references to warriors, warrior chiefs, war chiefs, Pine Tree chiefs, and sachems. This confusion also dulls the author's important point about factionalism within and among the tribes of the confederacy. Finally, Professor Graymont occasionally lets slip a remark that does less than justice to her subject. She writes, for example, that the Seneca were induced to break the League's neutrality by British "rum and trinkets" (p. 128). Surely historians must credit to the Indian the same motivational complexity they do to their white counterparts.

JAMES L. AXTELL
*Sarah Lawrence College*

EDWIN A. BEILHARZ. *Felipe de Neve: First Governor of California.* San Francisco: California Historical Society. 1971. Pp. 194. $12.95.

The history of the often vicious conflict between Church and state in colonial Spanish America has usually been decided against the king's bureaucrats. This has been true because the clerics wielded a more able pen. Nowhere was this fact better demonstrated than in California where Fray Junípero Serra had his Boswell in the person of Fray Francisco Palou. As a result Serra's adversary, Governor Felipe de Neve, has often been dismissed as "a meddlesome theorist" or otherwise portrayed in the worst possible light. This excellently researched and well-written biography, originally the author's dissertation, redresses the balance in favor of one of the "greatest of Spanish governors."

Felipe de Neve first administered the two Californias from Loreto (1775–77) and then from Monterey (1777–82). During his tenure the fledgling colony was greatly strengthened. Neve's many accomplishments are all treated in detail: he founded San José and Los Angeles, California's first towns, and the produce raised by their settlers started the colony on the path to self-sufficiency; he put the finances of the colony on a sound basis; he overhauled the army and improved the presidios; he built the presidio of Santa Barbara and aided in the establishing of three missions along the channel coast to close that important coastal link. Neve is best remembered for his famed *reglamento* that provided the basis of Spanish government in California for years to come.

Professor Beilharz, like most biographers, is partial to his subject and takes Neve's side in the famed quarrel with Serra. However, the author is always fair and just in the presentation of all sides of the controversy. (One new point is the fact that Neve was once an administrator of expelled Jesuit properties and came to California prejudiced against the clergy and the mission system.) In addition to the treatment of the major events of the Neve administration, there is substantial material on the problems of all Spanish colonial administrators. Especially well handled are the difficulties of the royal mail and the problems of communication along with the sheer burden of preparing the lengthy reports without secretarial assistance. Some of these reports were ten pages in length and often had to be handcopied in quadruplicate. In short, this book is a must for anyone interested in the history of California or colonial Spanish America.

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VAN BECK HALL. *Politics without Parties: Massachusetts, 1780–1791.* [Pittsburgh:] University of Pittsburgh Press. 1972. Pp. xvii, 375. $14.95.

Despite all the attention that Massachusetts politics has received in the generation since Robert Brown's *Middle-Class Democracy* (1955) appeared, Van Beck Hall's *Politics without Parties: Massachusetts, 1780–1791* treats a neglected but nonetheless crucial decade. His focus is state politics from the adoption of the state constitution, through the sharp internel conflicts of the late 1780s (Shays's rebellion and the ratification controversy), to the behavior of Massachusetts politicians in the first federal Congress. It is an important story, and if Hall's study were no more than a political narrative it would be a useful contribution.

But *Politics without Parties* is not simply a narrative. It is primarily a behavioral analysis, through time, of the political positions of town representatives and towns themselves. For this purpose Hall analyzed roll calls in the legislature and state-wide election returns exhaustively. His interpretation, while reminiscent of Jackson Main's *Antifederalist* (1961), is based on a systematic and carefully developed classification of towns that is new. Using various economic and cultural indicators from several years Hall has placed each of the 343 towns in a rank order that he calls the "commercial-cosmopolitan" continuum. Then he correlates the political stance of town representatives and towns with their level of commerciality and cosmopolitanism. The gross results are not startling—that towns like Boston, Salem, and Newburyport, for example, favored creditor-oriented fiscal policies, opposed Shaysite activity, and, finally, supported the Constitution, in contrast to poor, agricultural towns—but Hall's data are ultimately much more satisfactory than the impressionism that has held sway. Moreover according to Hall's analysis the great majority of towns, no matter where they ranked on the scale, were highly sensitive to economic issues and remarkably responsive politically from year to year. It is this explosive responsiveness which, in the absence of the "glue" of political parties, Hall believes accounts for the volatile character of the political system. Normally, Hall maintains, there were broad areas of consensus among all the towns and most divisive issues did not overlap so as to produce state-wide polarization; but in light of other circumstances the commercially oriented economic legislation of 1786 generated just such a division, and mechanisms of restraint short of force were inadequate. Subsequently, Hall argues, the electoral process moderated the conflict and the new federal government together with an improved economy would bail the state out of its crises and restore relative tranquility.

Both the method and substance of *Politics without Parties* deserve serious consideration from students of American politics before the Civil War. It is not easy reading, and the author sometimes speaks with oracular confidence and brevity to readers who may have doubts; but both the method and the data are presented with a clarity and precision that makes them readily comprehensible. Thus it is especially regrettable that the Pittsburgh University Press omitted Hall's three appendixes (including the rank list of towns and the measures) and that the author himself did not explore the relationship of his study to (among others) the work of James Banner, Lee Benson, Jere Daniell, Gordon Wood, and Ronald Formisano.

RICHARD D. BROWN
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DAVID F. LONG. *Nothing Too Daring: A Biography of Commodore David Porter, 1780–1843.* Annapolis: United States Naval Institute. 1970. Pp. xiv, 396. $12.50.

Few naval "heroes" can have had stranger careers than Commodore David Porter. Midshipman under Thomas Truxtun on board the *Constellation* during the quasi-war with France (Porter received his warrant two weeks before the Navy Department was established), lieutenant on board the *Philadelphia* during the Tripolitan War and then a prisoner of the corsairs, frigate captain in the War of 1812, one of the original navy commissioners, commander-in-chief of the West India squadron, and then commander of the Mexican navy, he spent the last decade of his life as United States chargé d'affaires and minister to the Ottoman Empire. In many ways, however, it was an unsuccessful career. As first lieutenant of the *Philadelphia,* he was partially responsible

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for her loss to the Tripolitans. On the *Essex* he captured a much weaker British warship, but his subsequent Pacific cruise ended in surrender to superior British forces after a battle he should have avoided. He was relieved of command of the West India squadron after violating the Monroe Doctrine by an unauthorized "invasion" of Puerto Rico. The subsequent court-martial led him to seek his fortune in Mexico, and there, too, he failed. Nor was his personal life more tranquil; perennially in debt, he was the father of ten children, a number of whom died tragically. During the final decade of his life Porter was separated from his wife and estranged from his eldest son.

Earlier biographies—the first by a younger son, Admiral David Dixon Porter—are laudatory and inaccurate; hence this work, which is based on thorough research and attempts an objective study of its subject, is welcome indeed. Long concludes that Porter's own complex personality was responsible for most of his difficulty, a judgment that seems amply supported by the evidence. One may question, however, whether Porter's impact on history was quite so important as his biographer believes. He may have been "the first American imperialist," but his efforts seem to have had little real influence on the actual course of events. While the book is quite well written, Long seems a bit uneasy on occasion with regard to nautical technicalities. A more serious fault is his habit of including so many sources in a single note that it is very difficult to determine the origin of a quoted passage. The illustrations are well chosen and reproduced; that illustration supposedly a picture of Commodore John Rodgers is actually that of his son, Rear-Admiral John Rodgers.

ROBERT ERWIN JOHNSON
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JOSEPH M. HAWES. *Children in Urban Society: Juvenile Delinquency in Nineteenth-Century America.* (The Urban Life in America Series.) New York: Oxford University Press. 1971. Pp. xvii, 315. $8.50.

RAYMOND A. MOHL. *Poverty in New York, 1783–1825.* (The Urban Life in America Series.) New York: Oxford University Press. 1971. Pp. xv, 318. $8.50.

Here are two solid books that trace institutional responses to social problems closely associated with the urban-industrial growth of nineteenth-century America.

Joseph Hawes's study is the more broadly based of the two, which is appropriate considering that to talk about juvenile delinquency is to talk about a society's notions of youth, family, order, and deviance as well as the more general patterns of socialization and norms that Hawes finds especially significant. Accordingly, he surveys ideas about wayward youth in literature, science, and popular culture, in both American and European contexts, all as a background—inevitably somewhat sketchy—for a finely focused study of the evolution of American institutions for juvenile delinquents during the nineteenth century. He traces the various phases—custodial "houses of refuge," more liberal reform schools, the emerging "placing out" system—but underscores the continuity in them, both in the understanding of children as junior adults and in the assumed centrality of environment as shaper of character and behavior. It was only at the end of the century, Hawes argues, with the development of the juvenile court, that a genuinely individualized and child-sensitive approach began to emerge, combining in a characteristically American fashion the new psychology of G. Stanley Hall and the folksy humanitarianism of Judge Ben Lindsey.

Raymond Mohl's study has a much narrower focus, also appropriate. It is an exhaustive case study of the administration of welfare and charity, public and private, in New York City between 1783 and 1825, as the city became industrial. By the latter date, he shows, institutions and patterns many have associated with a much later period were clearly established: New York had developed a fully articulated, quasi-scientific approach to the administration of aid and to the search for the causes and prevention of poverty itself. However, Mohl argues, the price for these advances was a shift from a general cultural sense of benevolence to a moralistic notion of poverty, one energized as much by a perceived need to restore social order and control as by humane sensitivity.

These thorough studies share three limitations, interrelated and perhaps inherent in the approaches the authors have chosen. In the first place, the study of social response can only

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partially reveal the objective conditions of social change. This becomes especially problematic in Mohl's study, which only occasionally attempts the analysis of poverty promised in the title. We learn little of the poor themselves; there is scant attention to the economic structure of New York's population and no real exploration of wage, income, and unemployment patterns. As a result the tracing of reform attitudes and institutional change seems cut adrift from its context, and the book does not benefit enough from the localized focus to justify the narrow scope.

This gap between conditions and response helps sustain another problem, which is the difficulty both authors have in moving beyond the perspective of the sources. Hawes and Mohl, to their credit, show a sensitive respect for the people and problems they deal with. But at times the need for greater analytic distance is glaring. Hawes, especially, mirrors uncritically the positivistic approach he describes and the cultural assumptions of those he studies. For example, with little analysis of any of the key terms, he argues that since most wayward children had immigrant parents, "their delinquency was a consequence of their foreign culture"; their parents "did not fully understand the values and mores of American society." Thus a promising analysis of cultural conflict and transmission across generations and group reduces to the familiar and dated assimilationist argument, which is itself presented rather than examined carefully.

A third limit of these works is the reluctance of the authors to reflect on the implications of their work beyond the subject area itself, or in the deeper dynamics they describe. Hawes does not fully explore the terms and processes of cultural transfer, and Mohl does not advance much beyond stating the now-familiar tension between humane motives and a manipulative concern for social order. One comes away with the sense that much more could have been said—about urban institutional behavior in general, and its relation to cultural structures and the dynamics of social change. But Hawes and Mohl have, in these thorough and professional works, opened to view the rich detail and complex structure of institutional development and thus have done much to make such speculation possible; it is perhaps just as well that they have left the reader relatively free to move in a variety of directions from the valuable vantage point they provide.

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DWIGHT F. HENDERSON. *Courts for a New Nation.* Foreword by TOM C. CLARK. Washington: Public Affairs Press. 1971. Pp. viii, 166. $6.00.

Legal scholars have begun to devote attention to courts other than the United States Supreme Court. Dwight F. Henderson has studied extensively U.S. district and circuit courts in the Federalist era: "The records of the lower federal courts are an extremely rich and virtually untapped source for the study of American constitutional and legal history."

Henderson explains that the need for federal inferior courts was not assumed by members either of the Constitutional Convention or the First Congress; in fact, there was considerable opposition from anti-Federalists who feared that such courts might render state tribunals impotent or unnecessary. The debates, both in 1787 and 1789, over the establishment of lower federal courts foreshadowed the ideological conflict between Hamilton and Jefferson and, moreover, the emergent partisanship of the 1790s.

The principal strength of this book lies in its interesting and detailed description of district and circuit court activities through 1801. Henderson gives us a collective portrait of judges, attorneys, and marshals. In some areas—such as his treatment of the Judiciary Act of 1789 and his description of well-known circuit court cases—the author builds upon, but does not improve, the analyses of Charles Grove Haines (*The Role of the Supreme Court in American Government and Politics* [1944]) and Charles Warren (*The Supreme Court in United States History* [1922]). But his exhaustive treatment of the kinds of cases district and circuit court judges heard gives us a complete picture of how the new courts contributed to national development. As Henderson says, "the courts anticipated the nationalist decisions of the supreme court under John Marshall."

In his last chapter Henderson states that

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"[e]ven though the establishment of the courts was entangled in the political quarrels of the federal period, the courts remained above politics in most instances." Regardless of what one means by "above politics," his conclusion is an understatement. Few legal philosophers or historians today maintain that courts are not a part of a nation's politics. Henderson's research demonstrates that district and circuit court judges alike became embroiled in significant political controversies: land disputes, revenue and tax collection, creditor-debtor conflict, interpretations of treaties with Great Britain, determination of relationships between state law and federal treaties and law, and, significantly, enforcement of the Alien and Sedition Acts. "The objective of the Federalists . . . [from September 1789] was to erase the dependence on state example and expand if possible the jurisdictions of the federal courts." With the operation of the lower federal courts and the passage of the Judiciary Act of 1801, as Henderson explains, the Federalists met their objective. But one cannot aver that this was accomplished above, or apart from, politics.

ROBERT G. SEDDIG
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*Journal of the Senate of Virginia. November Session, 1793.* Richmond: Virginia State Library. 1972. Pp. vii, 110. $4.00 postpaid.

JOHN A. WILLIAMS, editor. *Journals and Proceedings of the General Assembly of the State of Vermont, 1791–1792.* (State Papers of Vermont, volume 3, part 5.) Montpelier: Richard C. Thomas, Secretary of State. 1970. Pp. xviii, 263. $5.00.

Legislative journals of the early state period generally work only as guides to the business of an assembly. Petitions, reports, amendments, and executive papers are either excluded or only briefly summarized. But since the journals provide entry to the surviving legislative papers, they are a vital reference tool. Both these volumes belong to continuing series designed to make these rare sources more readily accessible. The Virginia State Library plans to publish eight years of senate proceedings between 1792 and 1803, and the 1793 journal is the third of the series. Vermont, having enjoyed modern editions of its assembly journals of the Confederation period for almost forty years, has decided to continue that series from 1791 to 1800. In Virginia's case, the new editions will fill all the chasms in the extant series of printed senate journals, leaving only eight scattered years for which no manuscript or printed proceedings have been found. As for Vermont, early printed assembly journals are scarce, and modern editions of her executive records (to 1836), general petitions (1778–99), committee reports (1778–1801), town charters, and early land records make access to well-indexed eighteenth-century assembly journalists essential.

The Virginia senate journal is competently, if rather unattractively, presented. Footnotes are rightly confined to peculiarities in the text. There is a useful summary of the senate's powers, a fine index, and a superb calendar of senate action on bills and resolutions. The editorial method is properly described and accurately applied. The reader is in good hands and can depend upon his text. But the Vermont journals rather worry me. The index is inadequate—bills, resolutions, reports, and committees are not referenced. The footnotes frequently duplicate information found in the journal and seem to have been inserted at whim. Above all, we are not told whether the text is taken from a manuscript or printed copy, nor are we given any statement of editorial policy.

It is fascinating to look at these two journals as examples of the striking differences between legislative practice in Vermont and Virginia in the first years of the republic. The little New England assembly shows none of that jealous and punctilious regard for its privileges that her Southern cousins exhibit. Major business is routinely referred to joint committees or a grand committee of assembly and council in which the governor is an active participant. Members of council appear in the assembly without the speaker's leave. The governor, assembly, and council of Vermont are essentially acting in concert. In Virginia, the constitution reserves almost all state appointments to an assembly dominated by the lower house, provides for annual elections of delegates and governor, rotates senators, puts strict limits on the legislative power of the upper house, makes the governor a creature of the assembly, denies the executive a share in legislation, and confines the money power to the house of delegates. Joint

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committees are used sparingly; the two houses communicate by formal message; and disputed amendments are sent back and forth rather than submitted to a conference committee.

In Virginia, as in other Southern colonies, the assembly had taken the British House of Commons for its model and sealed into the Revolutionary constitution all the rights it had claimed in a long struggle with Crown officials. This model of parliamentary government baffled Northerners accustomed to the informal traditions of the town meeting. "The constitution seems to be in general pretty well framed on the republican plan," one Northern visitor said of South Carolina's 1778 constitution. "Except one clause which displays their english prejudices wherein the sole right of levying taxes is vested in the house of representatives. . . . And the house of representatives are as tenacious of this unmeaning prerogative as the convention was preposterous in the initiation." How did two sections with such different constitutional traditions develop a standard form of government? Clearly, the process had only just begun with the foundation of the republic.

R. NICHOLAS OLSBERG
*Colonial and State Records of South Carolina*

DONALD B. COLE. *Jacksonian Democracy in New Hampshire, 1800–1851*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xi, 283. $10.00.

Considering Cole's endorsement of Arthur Schlesinger's battered interpretation of Jacksonian Democracy, it is appropriate that Cole's book appeared during the twenty-fifth anniversary of the publication of *The Age of Jackson*. Cole finds that the conflict thesis fits New Hampshire quite accurately, though I think he asserts more than he proves. Acknowledged as the nation's most Democratic state prior to the Civil War, New Hampshire obviously merits the attention lavished upon it by Cole. Surrounded by a sea of Whiggery (especially Massachusetts and Vermont), the Granite State remained solidly Jacksonian; why it did so is not made altogether clear by the author.

Cole probably errs in trying to stretch the origins of the Jacksonian movement in New Hampshire back to 1800; at least he does not build a very persuasive case. Moreover, he fails to illuminate the process by which the first party system expired and yielded to the second. The author seems more sure of himself as he launches, quite effectively, the Isaac Hill and Levi Woodbury *mariage de convenance* theme. Their sometimes unsteady and volatile alliance dominated state politics for almost three decades until younger men such as Franklin Pierce took over.

The Bank War quite naturally receives Cole's attention. He is firmly convinced that the Portsmouth branch controversy of 1829 signaled the genesis of the national dispute over the Second Bank. Despite his information about the entrepreneurial motivations of some anti-Bank Jacksonians, notably Hill and Woodbury, Cole insists that Schlesinger's interpretation is "closer to being correct" than Hammond's thesis.

The re-election of Jackson is the only presidential election analyzed, the weak rationale being that "the year 1832 marks the apogee of the Jackson movement while Jackson was President and the Bank War was hot" (p. 142). To be sure, Cole does an excellent job of probing New Hampshire's 1832 returns, but it is regrettable that he does not turn his analytical talents to more important presidential contests. It is interesting that his conclusions about support of the Jackson party conform to what has generally been found for other states.

Cole makes the perceptive point that New Hampshire Democrats were more interested in reforms than were their opponents. But Herbert Ershkowitz and William G. Shade, in their article on legislative votes in New Hampshire and five other states (*Journal of American History*, 58 [1971]: 591–621), contend that Whigs, rather than Democrats, had more enthusiasm for reforms. There is room apparently for further investigation on this important matter.

Despite its shortcomings, Cole's study makes a valid contribution as a monograph that must be reckoned with in the broader context of Jacksonian politics.

PAUL H. BERGERON
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C. VANN WOODWARD. *American Counterpoint: Slavery and Racism in the North-South Dia-*

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*logue*. Boston: Little, Brown and Company. 1971. Pp. 301. $7.95.

These ten essays are bonded together by their focus on slavery, racism, and the contrapuntal themes which have arisen from American debates on these matters over the last one hundred and fifty years. Though not avowedly so, many of the chapters take the form of essay-reviews; they are not therefore narrow, for they utilize the writings of the past and of the scholarly present as springboards for discussion which for the most part goes beyond previous confines.

Seven of the essays ( or versions thereof) have been published before. There are chapters on George Fitzhugh, Lewis H. Blair, and W. J. Cash—all interesting men and each eccentric to the climate of opinion of his day. "The Southern Ethic in a Puritan World" tackles the problem of the South's distinctiveness. And there are three essays focusing on the coming of The Nadir: a discussion of the abolitionists' stances on racial equality, a piece on the "Seeds of Failure in Radical Race Policy" which emphasizes the Northern fear of Negro migration from the South, and an account of the *Plessy* v. *Ferguson* decision featuring two people on the "right" side, Albion Tourgée, the militantly pro-equality carpetbagger, and John Marshall Harlan, the conservative Southern dissenter.

Of the altogether fresh essays, a discussion of Brazilian and Southern United States slavery which centers upon the work of Gilberto Freyre is the least original and stimulating. Much more striking is the attack upon a discrete problem: why, Woodward asks, was slavery in the United States demographically unique in the New World? In Latin America and in the Caribbean islands black slaves died much faster than they reproduced; only in the United States was there a large natural increase among the slave population. (The fact itself has been obvious for a long time, so obvious that American historians have blithely ignored it until Philip Curtin recently laid it out in quantitative form.) Woodward's admirably cautious and tentative elaboration of the possible reasons for this arresting phenomenon stresses the complexity of the issue. Climate alone won't explain it. Neither will Southern paternalism and benevolence. As the author points out, there is a possible explanation for everyone: if you don't like Southern benevolence you can always make the South into a giant breeding farm. "Perhaps the most useful contribution within the present range of possibilities," Woodward so characteristically puts it, "would be some cautionary observations on the dangers of simplistic solutions."

Anyone capable of writing that is capable of almost totally disarming criticism, which is what Woodward does in "The Strange Career of a Historical Controversy." He begins by pointing out that historians too often engage in adversary proceedings and too frequently have fallen into the "habit of being more concerned about winning the suit than about pursuing the truth." He then goes on to review a historical controversy which "began in a mild way with a series of lectures in the fall of 1954." The ensuing discussion of the historiography of that controversy is marked by a deliberately low-key, chivalrous generosity; anyone challenging him will have to shuck any tendency toward Yankee shrillness or be accounted an oafish boor. Woodward advances rather than reiterates: utilizing the "models" of van den Berghe and Philip Mason (neither of them Americans), he suggests that there has been with race relations in this country a movement from intimate-paternalism to distant-competitiveness (not his terms, of course—he does not, thank goodness, write that way).

His suggestions are above all sensible and judicious, and one hopes they will help close the "strange" career of this controversy. The fact remains, though, that he began it, and it need not have arisen. By settling on segregation as the key object of investigation both he and those who have pursued the matter since have been boxed into making (what seem to me) two crucial errors: (a) assuming that segregation has been central to race relations in this country and (b) trawling chronologically backwards for the "origins" of segregation. The second error is clearly derivative from the first.

Historical fallacies do not need cataloging as much as they need elimination; and the latter, clearly, is Woodward's aim. In the same spirit let me say that our basic mistake has been assuming race relations in this country to be somehow peripheral to our national experience, North and South. This nation's record of

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dealings with Afro-Americans, including the policy or practice of "segregation," has not been merely a wart; it has been and is a central attribute of our national culture, and, indeed, variously of the various nations populated largely by the migration beginning some five hundred years ago of Europeans overseas. Certainly for the United States, without an understanding of this fact (to quote the final sentence of one of these essays) "that would have been somebody else's history and not our own."

Thus one can easily object to where the author started some years ago, with the ontogeny of "segregation," but that is not now where he is headed. More than most of us he is grappling with the tragedy. He looks at it from an angle which seems to me geographically specific, unnecessarily so. But this is to say that individuals have a geographical standpoint from which they necessarily observe. And more than most people, Woodward knows that we are all caught by our history—not a new perception but one insufficiently recognized.

One can see these points most poignantly in his conclusion concerning Northern abolitionists: "And now that the old policy of 'containment' no longer holds, now that the fears of an inundation by Negro migrants from the South that obsessed the North of the 1860's have materialized a century later the continuity of plantation and ghetto is borne in upon us. It also seems rather more difficult than it was before to be confident in justifying the sacrifice of those 600,000 lives. It was a crusade to be sure, but a 'limited' one, and it does appear to have been corrupted and frustrated all along by an old sickness endemic among the crusaders." The disease was indeed endemic, but not solely "among the crusaders."

One Confederate or Union soldier died for every six slaves freed. It's not who counts but how and why.

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GERALD SORIN. *Abolitionism: A New Perspective.* (New Perspectives in American History.) New York: Praeger Publishers. 1972. Pp. 187. Cloth $7.50, paper $2.95.

GERALD SORIN. *The New York Abolitionists: A Case Study of Political Radicalism.* (Contributions in American History, Number 11.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xiii, 172. $9.00.

*The Letters of William Lloyd Garrison.* Volume 1, *I Will Be Heard! 1822–1835,* edited by WALTER M. MERRILL; volume 2, *A House Dividing against Itself, 1836–1840,* edited by LOUIS RUCHAMES. Cambridge, Mass.: Belknap Press of Harvard University Press. 1971. Pp. xxx, 616; xxxi, 770. $20.00 each.

WILLIAM B. HIXSON, JR. *Moorfield Storey and the Abolitionist Tradition.* New York: Oxford University Press. 1972. Pp. ix, 256. $8.75.

The civil rights and black nationalist movements have generated interest in old humanitarian causes of the nineteenth century, but the field remains strangely sterile. A decade ago C. Vann Woodward quarreled with what he called "the antislavery myth," but the note of caution went virtually unheeded. With only a few exceptions, scholars have presented their findings as if contemporary racial contests required the support of updated humanitarian scriptures. The books under review represent the latest portion of accepted canon. They share a common bias: abolitionism was worthy; American society (including many antislavery advocates) was racist; and slavery was sinful. What scholar would dare dispute these articles of faith?

These remarks do not imply that abolitionism was morally or politically tragic, racism commendable, and slavery benign. Nor do I mean that antebellum reform and reaction can tell us nothing about present politics. Long ago Tudor and Stuart scholars abandoned the Whig interpretation in order to inspect the interconnections of ideology, politics, and social stratifications. To overcome the toils of judgmentalism, all of us in antislavery studies may require a healthy kick in the pants from an American Trevor-Roper, that is, a scholar who dares to be wrong in the effort to stir productive controversy. Eugene Genovese has attempted this feat in provocative analyses of Southern antebellum history, but reform Yankeedom has been less well served. Aside from David B. Davis and a few others, most scholars in Jacksonian and reform history betray old liberal and New Left sympathies and show an unwillingness to accept the integrity of the past, to view one reform impulse or another in the

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context of the total society and the total reform milieu and evolution. Instead, a search for heroes and villains dominates, just as in the days of James Randall, though the roles assigned in the moral drama have been reversed for contemporary purposes.

The authors of the books under review are not themselves to blame for the malaise. It is a professional, not a personal problem. Gerald Sorin's *Abolitionism: A New Perspective*, for instance, is merely a symptom of trouble. The book is a competent, if dull summary of the latest opinions, but the subtitle remains a promise unfulfilled. Designed for undergraduates, the work retells the story of emancipation without much inquiry into the dynamics of reform and reaction or the reform strategies and lucky circumstances that enabled abolitionists to convince Northern rulers and followers that slaveholders, not reformers, posed the greater threat to Northern hegemony.

In his *New York Abolitionists*, on the other hand, Sorin is less easily excused. He uses conventional means to reach conventional answers. Abolitionists, we learn, were psychologically healthy, socially unremarkable, and morally impeccable. He may well be right, but when a figure as complex as Gerrit Smith passes Sorin's test of mental fitness without a blemish, one must demand some deeper probing before the case is closed.

A more positive note is appropriate for the careful work of Walter Merrill and Louis Ruchames in editing the William Lloyd Garrison correspondence. If antislavery studies are ever to move out of present doldrums, these volumes and the ones to follow will be indispensable. This belated tribute to the Boston editor enables scholars to judge for themselves the varying opinions about him. The letters also suggest the influence of transatlantic political, religious, and intellectual romanticism upon the Boston leader, as well as his rootedness in traditional Puritan culture. Aileen Kraditor's appreciation of Garrison's strategy, as it developed up to the fatal abolitionist convention of 1840, receives much support, even in regard to his sense of humor. Yet it is also clear that she overlooked the faults of his virtues in deference to present-day preoccupations. Garrison reveals herein the streak of unpleasant egocentrism and self-unawareness of which John L. Thomas complained in his brilliant biography. Merrill and Ruchames provide useful information in the elaborate notes, but the introductory materials are unstimulating. Rather than suggest new perspectives, they defend Garrison from earlier attacks, a rather barren undertaking.

Hixson's *Moorfield Storey* suggests a new approach, for we know too little about the influence abolitionism exercised in the gradual secularization of social welfare during the late nineteenth century. Storey, he reveals, helped to unite the old cause with racial humanitarianism and protest in the twentieth century. He argues that Storey was preserved from late Victorian jingoism and racism more out of innocence than design. Instinctively, he resisted Darwinian scientism, a fashion that led so many of his "independent" friends toward complacency and strenuosity. But if Charles Sumner's former law clerk held the torch of his mentor's idealism, he lacked the toughness of the Massachusetts senator. As president of the NAACP in its early history, Storey was gentle and humane. Yet he was not the kind to wrestle, like Garrison, in the ditch of agitation. Hixson, however, does not wholly resist the current temptation of presentism. Storey's lamentations over Philippine conquest, oil politics, and national racism invite instant application to the United States in 1972, but somehow the limitations of Storey's philanthropy and legal temperament are lost in the stress upon "relevance." Notwithstanding these comments, the book is important. It challenges the notion that abolitionism left only memories; it remained a force. The reader cannot help but admire the integrity of this Boston echo of the abolitionist past and the clarity and organization of his appraiser.

To return to the point made at the beginning, we must not become too preoccupied with abolitionism as a revolution still unfolding. The moral pertinence of Garrison and company hardly needs further elaboration. Instead, the Old Republic should be treated as the "world we have lost," to borrow Peter Laslett's phrase. The abolitionists helped to shape the modern world, but their New England traditions, proscriptive habits of mind, social origins, and contributions to some of the less happy aspects of nineteenth-century liberalism were as important in guiding the course of

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American history as their seemingly modern idealism. After all, their sectional chauvinism, missionary zeal, and general bellicosity may have had more than a casual relation to American imperialism and warlike righteousness, for all the talk of nonresistance and Christian humility. It borders on *déjà vu* to repeat Woodward's warning, but it still applies.

BERTRAM WYATT-BROWN
*Case Western Reserve University*

DAVID M. POTTER. *The South and the Concurrent Majority*. Edited by DON E. FEHRENBACHER and CARL N. DEGLER. (The Walter Lynwood Fleming Lectures in Southern History, Louisiana State University.) Baton Rouge: Louisiana State University Press. 1972. Pp. viii, 89. $4.95.

WILLIAM C. HAVARD, editor. *The Changing Politics of the South*. Baton Rouge: Louisiana State University Press. 1972. Pp. xxv, 755. $17.50.

David Potter had a gift for bringing new perspectives into focus. In his posthumous *The South and the Concurrent Majority*, based on the Fleming Lectures for 1968, we encounter once again a fresh and challenging interpretation. It would perhaps not push simplification too far, Potter wrote, "to say that one important way of understanding the period between the middle of the nineteenth century and the middle of the twentieth, is to recognize that, in one of its aspects, it was the Century of the Concurrent Majority." John C. Calhoun's *Disquisition on Government* expressed the essence of a negative power that the South maintained for nearly a century after his death. Calhoun simply failed to anticipate, and in fact rejected, the means: "zealous, even fanatical adherence to one political party."

Instead of Calhoun's finespun theory of a dual executive, two presidents, each with a veto, or any other formalistic contrivance, the effective device was the seniority system and the power of congressional committees supplemented by complex rules of procedure, the party caucus (mainly in its organizational function), unlimited discussion in the Senate, and the two-thirds rule in Democratic conventions. The seniority system jelled during the administration of President Polk, and when the Kansas-Nebraska Act devastated the Northern Democrats in 1854, the Southern wing acquired a dominant position in the congressional party, a position that it held, except for the interlude of Civil War and Reconstruction, until the New Deal.

The course of Reconstruction ironically strengthened the South's negative power. With the end of the three-fifths compromise the former Confederate states gained a larger representation than before and, in the phrase of James A. Garfield, slavery dead gained more power than slavery living. In 1881 Southern Democrats in Congress once again formed a majority faction in a minority party and so remained except when tidal waves of Northern Democrats swept into the House in 1883, 1885, 1891, and 1913. After 1933, however, when they became a minority faction within a majority party, they still fought an effective rear-guard action and suffered "something approaching unconditional defeat" only on the Civil Rights Acts of 1964 and 1968. The two-thirds rule went out in 1936, more recently the Senate has invoked cloture against Southerners and the House has curtailed their power in the Rules Committee, and now even the adherence of the region to one political party has weakened. "But if the tune is ended, a certain melody still lingers."

It lingers in the committees and in the informal coalition of Southern Democrats with conservative Republicans. The latter is treated in some detail in W. Wayne Shannon's essay "Revolt in Washington: The South in Congress" (actually in the House), which appears in *The Changing Politics of the South*. Shannon demonstrates that the rebellion against Franklin D. Roosevelt during the 1930s and 1940s grew into a far greater departure from party regularity during the 1950s and 1960s. Pursuing the kind of roll-call analysis that V. O. Key applied to the Roosevelt years in *Southern Politics in State and Nation* (1949), Shannon finds that roll calls on which majorities of Southern Democrats opposed majorities of Republicans declined from 70.5 per cent in 1933–45 to 48.0 per cent in 1951–67 and that roll calls which pitted Southern Democratic–Republican majorities against Northern Democratic majorities increased from 10 to 24.7 per cent.

A serious consequence of the enduring coalition (and the disproportionate number of Southern committee chairmen), according to

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Shannon, is that it "has rendered some of the most important aspects of our national policy processes all but incomprehensible to citizens who are less well equipped than scholars and pundits to fathom their complexities." It seems clear to Shannon "that a mode of politics so resistant to popular understanding ill serves a broad majority of the American people."

In Shannon's contribution and in other respects, comparisons of William Havard's *Changing Politics* with V. O. Key's classic *Southern Politics* (1949) will be inescapable, however much Havard may protest his "sublimation of the idea of being in competition with Key" or disclaim any pretense of equivalence. The new book is in fact a successor volume that owes its origin to Key's example. It is more limited in the scope of its research and coverage, but it can stand the comparison.

In retrospect Key's book seems almost a valedictory for the Solid South. Key described a situation that retained some degree of stability, despite the strains he duly noted. But Havard and others have had to cope with a situation of kaleidoscopic change. In 1949 Key dealt with what was still substantially a one-party system. Havard now writes that "it would be hard to categorize the politics of the South as anything but a 'no-party' system if we were compelled to bring the region under a single rubric."

The new taxonomy of Southern politics, according to Havard, "cannot now be reduced to a single major classification of the eleven states of the South, with subspecies being identified in categories of factional competitiveness." All eleven states deserve separate designation, he suggests, but he does adopt three general categories, roughly according to the degree of equilibrium each state has reached: the evolving states, the wavering states, and the protest states. Essays on the several states are grouped under these three headings.

Among the "evolving states" Ralph Eisenberg finds Virginia approaching a stable two-party system, Manning J. Dauer concludes that Florida now has more in common with other rapid-growth states (Nevada, Arizona, California) than with the rest of the South, Lee S. Greene and Jack E. Holmes argue that Tennessee has entered the orbit of two-party border states, and O. Douglas Weeks tells us that Democratic bifactionalism has inhibited the growth of Republicanism in Texas. Among the "wavering states" Richard E. Yates finds Arkansas in a condition of suspended ambivalence subject to change if either party can get organized, Joseph L. Bernd sees Georgia drastically changed by the growth of Republicanism and black voting, but Preston Edsall and J. Oliver Williams find that political change in North Carolina has been "more continuous than dramatic." In the "protest states" Donald S. Strong finds white Alabamians "alienated from the nation, not merely from the Democratic Party," Charles N. Fortenberry and F. Glenn Abney show a Mississippi somewhat changed but still dominated by the race issue, Perry Howard sees a decline of bifactionalism in Louisiana, and Chester Bain presents a picture of South Carolina so changed as to be near the "wavering" status.

Havard's book parallels Key's in its state-by-state surveys of recent developments and in its informative treatment of the conservative coalition in Congress. Beyond that, however, it lacks extensive topical syntheses like those Key developed. There is, for instance, no extended narrative or analysis of presidential politics (except state by state), nor of the ongoing political rebellion or the Southern Strategy that are comparable to Key's analyses of the party bolts in 1928 and 1948 or his description of Southern Republicanism. We get little or nothing on the mechanism and procedures of party politics, campaign finances, the character of the electorate, or restrictions on voting (or what would be more pertinent now, the decline of restrictions and the rise of the black vote), except again state by state. And we miss the flavor of the "inside story" that Key developed from the extensive interviews in every state, conducted by Alexander Heard and Donald Strong.

Two readable and informative essays by the editor bracket the book and tie it together: an introduction that establishes the background of tradition and change and a conclusion that summarizes the individual pieces and offers some generalizations. The book, however, lacks the unity of style and treatment that was imposed by the single direction and authorship of Key, although the essays maintain a high standard of excellence. The contributors are political scientists, with the exception of one historian and one sociologist, and each is a resi-

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dent of the state about which he writes, with the exception of two.

The research is mainly in printed sources. There is some use of aggregate and survey data but the methodology involves little mathematical complexity. The state essays and the summary present narratives of events since about 1949 together with evaluations of trends and changes. The essays are without exception cogent and readable, if not always sparkling, and readily comprehensible to the general reader. They afford to specialists a compendium of information about the 1950s and 1960s that is available nowhere else in such convenient form.

GEORGE B. TINDALL
*University of North Carolina, Chapel Hill*

JOSEPH S. O'FLAHERTY. *An End and a Beginning: The South Coast and Los Angeles, 1850–1887.* (Exposition-Lochinvar Book.) New York: Exposition Press. 1972. Pp. 222. $7.50.

California's chroniclers have given less attention to Los Angeles's transition from "queen of the cow counties" to world-renowned metropolis than today's third largest American city deserves. The region still lacks an outstanding full-length history, although several scholars, notably Robert G. Cleland, Glenn S. Dumke, W. W. Robinson, Remi Nadeau, and Robert M. Fogelson, have researched important aspects of its late-nineteenth-century development.

O'Flaherty's volume presents little factual material not covered by these authors, yet his synthesis accurately interprets the major trends on the "South Coast" during its formative years. He describes the violence of outlaws, vigilantes, and anti-Chinese racists and dramatically presents the local Indians' fate, the struggles to validate Mexican land titles, the clashes of stockmen with farmers, and the feuds among railroad kings seeking a transport monopoly. The author not only ably details droughts, floods, pestilences, and earthquakes, but also the blessings of equable climate, rich soil, strategic location, and valuable minerals. There are undeniable heroes in his roster of stage and trolley promoters, bankers, silver and oil developers, sheep and cattle ranchers, water seekers, and realty boomers.

Although O'Flaherty includes much colorful data on social life and lucidly explains the technology of ranch, farm, mine, oil field, railroad, and irrigation project, his abundant detail occasionally deflects attention from the superhero, southern California itself. His volume's rather loose structure may occasionally puzzle the nonexpert reader—the book's potential user—as to why certain episodes are included in a particular chapter. The illustrations are excellent and appropriate, but they should have been accompanied by maps of railroad, trolley, and stage lines. The author's talent for depicting human interest would have been well employed in delineating contributions of cultural and political leaders and tourists and health seekers who helped build his favored area.

O'Flaherty concludes with the boom of the eighties, an appropriate finale; for once its multifaceted impact had metamorphosed southern California, the region's modern outlines had emerged and it ceased to be the singular frontier here surveyed.

JOHN E. BAUR
*California State University, Northridge*

JOHN F. REIGER, editor. *The Passing of the Great West: Selected Papers of George Bird Grinnell.* New York: Winchester Press. 1972. Pp. 182. $8.95.

Now that Americans are expressing a renewed concern about the welfare of their environment, a complimentary interest is growing about the origins of the conservation movement. With this thought in mind John F. Reiger, a member of the faculty of the University of Miami (Florida), has edited a memoir and other papers of George Bird Grinnell, a pioneer conservationist and ethnologist who exercised considerable influence upon the first environmental programs of the federal government.

Grinnell (1849–1938) could hardly avoid an interest in the environment. He grew to manhood in Audubon Park on Manhattan Island, hunted with the grandsons of John J. Audubon, and attended the school of the widow of the great naturalist. Travels in the West in the 1870s confirmed his interest in wildlife and Indian culture. By 1883, where this memoir ends,

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Grinnell had begun to publicize his fear that commercial hunters were annihilating American game. Many sportsmen shared this view. As editor of the popular outdoor journal, *Forest and Stream,* Grinnell "consolidated the inchoate dissatisfaction among sportsmen . . . into a crusade to husband [America's] natural resources." Among these disgruntled sportsmen was Theodore Roosevelt, who, according to Reiger, "incorporated" the ideas of Grinnell into his philosophy of conservation.

Reiger has opened a fruitful avenue to the sources of early conservation, and he has demonstrated that George Bird Grinnell was an "originator and amalgamator of ideas" of the movement. He allows room for no other crusaders, however, when he declares flatly that Grinnell's statements "*prepared*" [his italics] Roosevelt for the environmental plans of Gifford Pinchot. This declaration is especially disconcerting when Reiger makes no effort to reveal the intellectual ties between Grinnell and the Republican president. In addition maps are inadequate and, in some instances, indiscernable, a serious shortcoming since the book describes many of Grinnell's expeditions to the West. Nevertheless, photographs, notes, and a select bibliography make this volume a worthwhile contribution to the literature of conservation.

LARRY D. BALL
*Arkansas State University*

DAMON WELLS. *Stephen Douglas: The Last Years, 1857–1861.* Austin: University of Texas Press. 1971. Pp. xvi, 342. $10.00.

This biographical presentation of Stephen A. Douglas's last years is a series of interpretive essays linked by narrative accounts of intervening events. The Douglas-Buchanan feud is used to establish the national setting and introduce the principal actors. The key interpretations are of the Lincoln-Douglas debates, which are displayed more in essence than in substance, and of popular sovereignty in theory, practice, and perception as the cardinal context for the debates. After narration has bridged the gap to 1861, the author appends a curious interpretation of the senator's joining the throng of Democrats who supported Lincoln's policy after the firing on Fort Sumter. Perhaps to provide a heroic exit for Douglas, Wells asserts that "the moment of his true greatness had only begun" (p. 258), that "Douglas had found in the preservation of the Union a cause worthy of his outsized talents and energy" (p. 296), and that "the greatest chapter in Douglas' life remained unwritten" (p. 297).

The author explains that his interest was aroused by a 1959 Broadway dramatization of the Lincoln-Douglas debates, which left him "with the uneasy feeling that the playwright felt that in order to make Abraham Lincoln a hero, he had to make Stephen Douglas a villain" (p. xi). Assiduous craftsmanship and fervid rhetoric reveal intent to draw the reader into an emotional apprehending of the drama. "The Lincoln-Douglas Debates settled nothing and they settled everything with a terrible finality. . . . The debates were a prologue, yet they held within themselves the essence of the whole tragic story of slavery, secession, and civil war" (p. 81). Judgments have been made of men and events without fear or favor, to use a hackneyed expression of the day. "James Buchanan was a good man and a bad President" (p. 12). "He would have made an excellent county judge, a good mayor, and a fair governor" (p. 13). "The only acceptable defense for a nonethical approach to majority rule [popular sovereignty] is the contention that the majority will always choose the just course" (p. 64). The credibility of a countless array of judgments rests rarely on evidence offered but to an exceptional extent on faith in the author's perceptiveness and diligence. Here, for a first book, is an attempt to distill essence in a manner more to be expected from a Roy Nichols or an Avery Craven.

This portrayal is an instructive exhibit for the perennial debate about the proper purview of historical writing. "Artistic and moral sensibilities" surely affected its conception (John Higham's phrase quoted by David Donald, *AHR*, 77 [1972]: 452). The "common reader" can as surely perceive "that familiar and unfamiliar elements have been put into intelligible relation by someone who gives tokens of trustworthiness" (Jacques Barzun, "History: The Muse and Her Doctors," *AHR*, 77 [1972]: 46). For a serious student of these crisis years, nonetheless, more questions are raised than are answered by the unhesitating affirmations, how-

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ever artistically formulated or morally disciplined.

THOMAS B. ALEXANDER
*University of Missouri, Columbia*

GEORGE H. MILLER. *Railroads and the Granger Laws.* Madison: University of Wisconsin Press. 1971. Pp. xi, 296. $12.50.

This is an excellent example of the solid, specialized, painstaking, straightforward, perceptive monograph upon which general works in American history should be based. It was not written for the general reader. It is not spectacular in style or content. It is worthwhile. It is useful.

There is not much revisionism here. But there has been reinterpretation over the years of many facets of the Midwestern experience, and Professor Miller has skillfully used newer perspectives and added a few of his own. In agreement with most contemporary students, he argues that Granger laws were, in reality, neither radical nor primarily agrarian. The Granger movement was composed of various interests: agrarian, political, business, and sectional. Miller modestly asserts that his emphasis is upon railway fares and freight rates, but the scope of his work is broader, dealing at least briefly with the wide variety of action and interaction among the railroads, the states, federal government, and special interests.

The book is simply structured in nine chapters and a short summary. The stage-setting segments concerning the railroad problem, development of rate law, and the mixed motivations of Granger era lawmakers are adequate. The final two chapters surveying the Grangers and Granger laws and considering public policy and private rights are excellent summaries. But the heart of Miller's contribution is in the middle four chapters: on Illinois, emphasizing the development of rate regulation; Iowa, stressing economic influences on law; Minnesota, dealing with the balance of promotion and control; and Wisconsin, concerning political influences. The author makes it clear that all these factors were a part of the regulatory scene in each of the states, but he skillfully points up one and then the other to avoid as much repetition as possible.

End matter is extensive. Notes follow the text, but there are sufficient guides at the top of pages to make them more readily usable than most so located. A bibliographical essay of some twenty-five pages, useful to the student of general railway affairs, indicates familiarity with a considerable body of literature not cited in the text.

ROBERT L. PETERSON
*University of Montana*

DAVID M. EMMONS. *Garden in the Grasslands: Boomer Literature of the Central Great Plains.* Lincoln: University of Nebraska Press. 1971. Pp. xi, 220. $10.00.

Changing the image Americans held of the central Great Plains from that of an uninhabitable desert to that of an Edenic garden required a massive and often irresponsible promotional effort. This theme has been dealt with several times, notably by Henry Nash Smith. David Emmons's contribution to the subject is a thorough analysis of the methods used to exploit a nation's desperate desire to maintain the idea that the Western frontier served as a safety valve for the less fortunate of the East and Europe. In highly readable fashion the author describes the campaigns of the railroads, the states, and the local communities, with emphasis on the methods developed for Kansas and Nebraska. A stroke of advertising genius provided the slogan that became the enormously influential promotional device—rain follows the plow. Basing their advertising largely on wishful thinking, with some support from scientists, the promoters persuaded potential immigrants that cultivation of the soil would lead to increased rainfall on the semiarid plains. In spite of all the physical evidence to the contrary thousands of settlers came. The results in many cases were tragic.

The author effectively demonstrates the close relationship between the national ideology of progress and the moral and spiritual values that supposedly would result from westward expansion. Less convincing is his assumption that Western urban growth played no part in the promotion of the Garden of Eden image. Perhaps the problem stems from the author's exclusive concern with land promotion, for the scope of the study is more narrow than the sub-

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title suggests. As a result of this approach only certain aspects of a promoter's ideas are described, as in the case of William Gilpin. As Charles Glaab has shown, Gilpin had a vision of Western development based on great urban centers as well as on rich farmlands. Then, too, much of Western land promotion originated from Western towns and cities that believed their growth depended on the establishment of prosperous agricultural hinterlands. Much of their promotion combined idealized pictures of both rural and urban life. Thus the significance of land promotion becomes clear only when it is seen in the perspective of Western promotion in general.

GILBERT STELTER
*Laurentian University*

TAMARA K. HAREVEN, editor. *Anonymous Americans: Explorations in Nineteenth-Century Social History*. Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xxii, 314. Cloth $9.95, paper $4.50.

Readers wishing to examine in compact form recent themes and tendencies in American social history will find a welcome sample in this volume. Ten essays, most dating from about 1969 and written for publication here, cover a wide range of nineteenth-century subjects, generally in an exploratory way and with a determinedly "up-to-date" tone and intent.

What, if anything, gives consistency to the collection? Tamara K. Hareven, its editor, in her brief introduction offers a few nominees. One is "forgotten dimensions" uncovered by investigating "groups in American society which have largely escaped traditional historical inquiry" (p. vii), anonymous in this sense though not in every case poor and powerless. "Process" is another defining characteristic, since in "traditional history . . . all movement has been treated as *terminal*" (p. viii, italics in original). But neither "forgotten dimensions" nor "process" really applies to all the essays in the collection. Martyn J. Bowden's study of "popular images of the plains," for instance, is in essence a carefully structured (and quantified) form of intellectual history, indicating just what imagery probably was available to what audiences of potential westward migrants; but it tells us little about any group in American society. "Process" likewise is absent from David Grimsted's provocative and valuable article on "Melodrama as Echo of the Historically Voiceless." One other unifying notion, "experience," is echoed by editor and contributors. To seek "experience" means here to go beyond elite points of view to discover how all manner of persons within society lived life or felt it (for the collection includes both behavioral and "cultural" essays). But so permissive a definition would also admit the "taxonomic or descriptive" (p. viii) tendencies of the older social history, against which Hareven quotes Schlesinger, Sr.

However hard to define, the sample of essays is an ambitious and good one and is valuable in revealing both the best and the worst tendencies of the newer social history.

Methodological awareness is such a virtue. Grimsted's brief article argues persuasively that we must recognize in the esthetic debasement of melodrama its essentially "religious" quality in democratic society and use repeated melodramatic themes as clues to "popular aspirations and uneasiness" (p. 82). Grimsted does not carry this analysis very far here, but Lawrence W. Levine, while also stressing method in utilizing the survivals of slave songs, says much more than "how to." Levine argues that slaves were not thoroughly deculturated; that by the incorporation of aspects of African culture within their Christianity the slaves' "personalities" were protected from "some of the worst ravages of the slave system" (p. 125); and that though slaves offered no "political" opposition to slavery "prepolitical" opposition was inherent in "the sacred universe created by slaves as a serious alternative to the societal system created by southern slaveholders" (p. 125). Methodological awareness also informs Paul B. Worthman's investigation of working-class mobility in Birmingham, Alabama, one of just three quantitative essays. Worthman's well-designed stratified sample permits systematic examination of the relationship between the ongoing processes of industrialization and the subjugation of the blacks in the New South.

When simple enthusiasm for "experience" overcomes concern for method the essays suffer. Leon F. Litwak's evocation of the coming of freedom to the slave South deploys magnificent quotations, but to little analytic end. Timothy L. Smith's interesting thesis—that the demo-

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cratic "lay initiative" tradition of Church organization rather than hierarchical structure was transplanted to America from Eastern Europe—is no less institutional in focus than the acculturative model it seeks to supplant. Richard Sennett's discussion of "Middle-Class Families and Urban Violence" is unspeakably careless of canons of historical proof, giving, for example, next to no evidence for the existence of the phenomenon—"[self-]isolation, through garrisons and police patrols" (p. 289) —it purports to explain.

Excellent essays on youth by Joseph F. Kett and on the mental hospital by Gerald N. Grob, together with an attempt at a synthesis of mobility studies by Stephan Thernstrom and Peter R. Knights round out this volume. The last-mentioned is particularly significant, for in it we begin to glimpse how these stabs of light into dark corners suggest a picture. Thernstrom and Knights illustrate the beginnings of maturity of a branch of history the vigorous youthfulness of which is demonstrated by *Anonymous Americans.*

JOHN MODELL
*University of Minnesota, Minneapolis*

ANDREW M. GREELEY. *That Most Distressful Nation: The Taming of the American Irish.* Foreword by DANIEL P. MOYNIHAN. Chicago: Quadrangle Books. 1972. Pp. xxviii, 281. $7.95.

This well-integrated collection of essays toward a history of the Irish in America fulfills the author's expressed intention to offend everyone: the professional Irishman; the person who admires Joyce, Bernadette Devlin, or the Berrigans; among the non-Irish perhaps especially the reformers and journalists of Cambridge (not Pat Moynihan who contributes a preface), "Chevy Chase, and Midtown"; among Catholics not only the hierarchy, but also the "kooks" and the "pale, cliché-mouthing imitations of the academic intellectual." Truly Father Greeley is the James Cagney of ethnic studies. The blows come in flurries, and he usually saves one mystery punch for the bell. Perhaps, he concludes one chapter, while the Irish have been "that most distressful nation," they have also been "that most happy people"; perhaps the Irish have not been "tamed" at all, or maybe they were tamed centuries ago by Christianity or by the English. Probably that is bad, or is it good?

He has the avidity of a Cuchulain about sources. Anthropological scholarship, NORC statistical surveys, faculty-club gossip, the speculations of psychiatrists about national character, his own experience in South Chicago are all summoned to help him plot the coordinates of the curve of Irish experience in America. That curve goes this way. The Celtic era established the most admirable traits of the Irish—a "fierce faith in life," a "passion for freedom," and "the wit and the ability to tell a tale." It's a bit hard to tell what he thinks the impact of Christianity was, but he leaves no doubt that the establishment of effective English control was deleterious to Irish character and conduct. Greeley then skips ahead to the famine migrations, and to the American scene, which for at least one hundred years he judges to have been uniformly unfriendly. On their own the Irish slowly and painfully worked their way up to lower-middle-class status by the early twentieth century; the onset of the depression embittered another generation, blocking as it did still further upward mobility. But war and the return of prosperity helped the Irish win upper-middle-class status, Greeley contends, whether measured by occupation or income, residence or education.

He acknowledges, of course, that to plot such a curve there must be one or more alternate paths; with some irritation he contends that the Jews should no more be "the" reference group for the Irish than the Irish for the blacks. So he tends to fall back on the WASPs, surely a somewhat heterogeneous group. But in his statistical arrays all these and others appear. It is on the basis of such comparative analysis that Greeley is able to contend that, contrary to the "liberal" mythos, Irish-Americans turn out to be neither conservative in foreign affairs nor especially bigoted in areas like civil rights.

Still, Greeley is despondent that the "rise" of the Irish has, if anything, intensified what he regards as a predominant joylessness, whose immediate cause is a set of family values and practices that makes mothers inhibited in their sexual life and tyrannical toward their children; fathers fleeing into the nearest bar as sol-

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ace from success in the office and gloom at home; young men preoccupied by the possibility of failure; young women seeking in religiosity more sedative than good news.

Patently these are difficult judgments to be confident about, but he who would write plausible social history—and even more certainly ethnic history—must cope with the questions involved. Greeley should not be faulted for being not wholly persuasive. I can't help wondering, though, if the phenomena he is dealing with stem from the Celtic background, Irish Catholicism, the early American experience, the relatively recent rise in status, or something else. I can imagine Father Greeley chuckling over my bewilderment.

ROBERT DOUGHERTY CROSS
*University of Virginia*

HARALD S. NAESS and SIGMUND SKARD, editors. *Studies in Scandinavian-American Interrelations, Dedicated to Einar Haugen.* (Publications of the American Institute, University of Oslo. Americana Norvegica, Volume 3.) Oslo: Universitetsforlaget. 1971. Pp. 390. $15.00.

In this *Festschrift,* as in so many of this genre, the editors seem to have sought essays to indicate the diversity of the honored one's interests. The title bears some relationship to the contents, but the implied theme is not fully realized. As is usual, a list of the writings, including even book reviews, of Einar Haugen, professor of linguistics and Scandinavian languages at Wisconsin and Harvard, is appended.

Perhaps the most substantive essay in the book is Ingrid Semmingsen's "Family Migration from Bergen, 1874–92." The article is based on the required lists of all emigrants from Bergen, starting in 1874. The meticulously researched essay goes into the sources of the emigrants, both urban and non-Bergen, and also deals with the problem of stages of emigration. The essay is a good example of the newer approach to emigration studies in the Scandinavian countries.

Other essays dealing primarily with emigrants are Franklin D. Scott's discussion of the divided loyalties of the emigrants, Kenneth O. Bjork's account of the Bella Coola settlement of Norwegians in British Columbia, Sten Carlsson's supplement on the period 1900–10 to his earlier study of the 1890s on Scandinavians in Minnesota politics, and Lloyd Hustvedt's study of the origins of the Norwegian-American Historical Association. Although Haugen's contributions to emigration history were minor, these five essays should be useful to historians of American immigration.

Of the twenty contributions in the volume ten have some relationship to Haugen's principal field of linguistics and literature. Two deal with linguistics, one on a minor problem of New Sweden place names by Gösta Franzén, the other by Walter Johnson on the American-Swedish language as used in the Chisago Lake settlement in Minnesota. The literary essays are Per Seyersted's study of a minor aspect of H. H. Boyesen's views, two thoughtful studies of August Strindberg by Harald Elovson and Carl L. Anderson, an essay on American influences on the Norwegian novelist Johannes V. Jensen by Niels Ingwersen, George C. Schoolfield's essay on the influence of Edgar Lee Masters on Elmer Diktonius, a study of Aksel Sandemose's Canadian novels by Asmund Lien, Harald S. Naess's essay on "American Attitudes to Knut Hamsun," and Bertil Nolin's rather limited study of the reviewing of Scandinavian books in American journals.

An essay of general interest is Marion J. Nelson's account of the Norwegian-American painter Herbjorn Gausta, illustrated with twenty-eight of Gausta's drawings and paintings. The contribution by James L. Larson on "Linné's Students in the Americas" will interest students of colonial science. Per Sveino's study of Kristofer Janson adds to previous essays by Nina Draxten and adds a dimension to the history of American Unitarianism. Finally, there is a tribute by Arne Kildal and a bibliography by Thor M. Andersen for Thorstein Jahr, a bibliographer at the Library of Congress and a researcher on some aspects of Norwegian-American history.

The volume, the third in a series published by the American Institute of the University of Oslo, will probably not reach many American readers, but it merits the attention of students of Scandinavian-American history. There is no index.

CARLTON C. QUALEY
*Carleton College*

LARS LJUNGMARK. *For Sale—Minnesota: Organized Promotion of Scandinavian Immigration*

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*1866–1873*. Chicago: Swedish Pioneer Historical Society. 1971. Pp. 303.

EMORY LINDQUIST. *Vision for a Valley: Olof Olsson and the Early History of Lindsborg*. (Augustana Historical Society, Publication Number 22.) Rock Island, Ill.: the Society. 1970. Pp. xiii, 138. $4.95.

The Swedes have awakened to the realization that emigration was a significant chapter in their own history. A group of twenty-odd scholars is producing rapidly at the University of Uppsala, and valuable studies have come from Lund and this one from Gothenburg. Archive centers are active in Karlstad and Vaexjoe, and the phenomenal success of Moberg's filmed epic attests a wide public interest. Three recent dissertations have been done in English, two in collaboration with the Swedish Pioneer Historical Society, one with the Chicago Historical Society.

Docent Ljungmark, who earlier did a popular radio series on emigration (published as *Den Stora Utvandringen* in 1965), has done this solid study of how Minnesota was "sold" to the discontented of Northern Europe, especially the Scandinavians. It relates the efforts of the State Board of Immigration, the colonization activities of the land-grant railroads, the rivalry of shipping companies and private interests. The author has pursued these often conflicting purposes in the archives of the state, the Jay Cooke papers and other railway sources, pamphlets, newspapers, and personal records.

In 1858 the first state legislature passed an "Act to Encourage Emigration." Propaganda was issued, including a pamphlet on "Minnesota for Invalids." The main actor in Minnesota's game plan was Colonel Hans Mattson, a Swedish immigrant who gained prominence during the Civil War and who "in himself combined the dominant trait of a wide-awake American with the racial characteristics of the Scandinavian." Private papers, such as Mattson's letters to his wife, expose the personal projects with which this official agent sweetened his contracts with shipping companies, railroads, and the state. The colonel was personable and cashed in on the prestige with which his title endowed him in Swedish eyes. He wrote articles, organized emigrant groups, and planned colonies. Because of the diverse forces that affected emigration it is impossible to measure each component precisely—but Minnesota did fill up with Scandinavian immigrants.

While some of the English usage in this volume is awkward it is certainly easier to read than Swedish for most Americans. They can be grateful, for it is a carefully researched investigation that throws new light into shady corners.

Emory Lindquist's biography of Olof Olsson is a different kind of book, focused on a smaller community of which he wrote before in *Smoky Valley People* (1953). Pastor Olsson came to the infant parish of Lindsborg, Kansas, in 1869 and quickly became an influence beyond the church—he was elected county superintendent of schools and representative to the state legislature. Firm in the faith, he fought both Grangers and Farmers' Alliance because they were secret societies. Lindquist combines understanding of the community in which Olsson grew up with an appreciation of the "vision" of the pastor. The reader gets a glimpse of the threat of prairie fire and grasshoppers, and also a bit of schmaltz, such as the childish writing of little Anna about the homesickness for Sweden of her parents.

FRANKLIN D. SCOTT
*The Honnold Library, Claremont*

RUDOLF GLANZ. *Jew and Italian: Historic Group Relations and the New Immigration (1881–1924)*. New York: [Ktav Publishing House.] 1971. Pp. 232. $10.00.

As the title implies the author has attempted to portray group relations between Jew and Italian from 1881 to 1924. The essence of the book is revealed in an introductory chapter in which Glanz states that the Italian and Jewish immigrations differ "in respect to their character and significance of the two peoples' concerns." Unlike the Italian migration to New York, the large migration of Jews to that city signaled a transfer of their "center of settlement to the North American continent." Moreover, the Italian migration "lagged behind" that of the Jews. These factors enabled the Jews to root themselves more quickly and effectively than was possible for the Italians. By

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1924 when the consolidation of "American Israel" had already taken place the coming together of Italians was still far off.

A major point emphasized is that there is little evidence of any industrial or social tension between Jew and Italian. As Glanz phrases it: "Within the triangle of Jew-Irish-Italian, the predominant stock of industrial wage earners among the ethnics, currents of tension are apparent only in two relationships: Jew-Irish and Irish-Italian, whereas the Jew-Italian association remained free of tension" (p. 10). The bibliographical ramifications of these facts are important to historians of American immigration. The conflict between Jew and Irish focused attention on their relationships that have been amply documented. Whereas the Jew-Italian encounters, perhaps because of the spirit of amity and cooperation that characterized them, are more difficult to uncover.

Glanz's work is commendable in that he has drawn his study from an impressive array of journals, foreign language newspapers, labor publications, reports of social work agencies, memoirs, and countless areas, major and minor, that brighten the "twilight in the sources." A number of his viewpoints are fresh and incisive, especially when he is dealing with the "Contemporary Evaluation of Jew and Italian" (ch. 27) and "Cultural Heritage and Traits" in chapter 12.

But there are a number of caveats. The book is in need of editing. There are twenty-eight chapters, one of which is only a half page, in 143 pages of text. There is a part 1, "Immigration and Economic Adjustment" and a part 4, "The Jewish and Italian Immigrant in the American Society." One part is not numbered, "Ethnicity and Acculturation." Another is apparently missing. In the chapter on criminality much of the material relegated to the footnotes could have been put to better use in the text to describe the role of Italian and Jew, rather than have the chapter read like a rehash of the often told story of the New Orleans Mafia incident. In addition, frequent and lengthy quotations make for difficult and awkward reading and obscure what the author himself has to say. The misspellings are not only annoying but, in one case, led to considerable difficulty for librarians who were trying to locate an item mentioned in the bibliography.

Most important, Glanz's treatment of the Italian immigrant is inadequate. Relying on a minor secondary source he attributed a schism among Italian societies to the fact that an Irish politician, James E. March, was chosen to head the Columbus Day parade in New York City in 1910. In fact, March was the Italian-born Michale Maggio and well known in his day as the most important *padrone* in New York. Glanz's handling of issues in labor and education gives no sign that he is familiar with the important findings of Rudolph Vecoli, Edwin Fenton, or Leonard Covello. Reiterating progressive views that pictured the Italian woman absolutely subject to male authority the author fails to consider the possibility that, in Joseph Lopreato's phrase, "the Italian patriarchal family is more fiction than fact." Ignoring Humbert Nelli's work on Italians in Chicago Glanz misses the point that Italians, not only Jews, joined together in societies to meet the new demand of the times.

Despite these reservations those interested in immigration history will want to carefully cull this volume and also take note of the difficulties one must encounter in attempting comparative studies.

LUCIANO J. IORIZZO
*State University of New York, College at Oswego*

GEORGE J. PRPIC. *The Croatian Immigrants in America.* New York: Philosophical Library. 1971. Pp. xiii, 15–519. $11.95.

Croatia is one of the sacrificed nations of the world, victim of the illusions of traditional federalism that puts nations together before they attain independence, as if a marriage with a slave, who attains freedom only through this union, could be successful. Fernand Braudel in his famous book on the Mediterranean in the sixteenth century wrote about Croatia: "The most striking scar of the Mediterranean countries is between East and West . . . this steadfast barrier which runs between Zagreb and Belgrade. . . . Even today in Croatia, mingled with many other heritages, an Italian way of life is perpetuated; although undoubtedly of a very old fashioned Italy" (*La Méditerranéen et le monde méditerranéen à l'époque de Philippe II.* [Paris, 1966], 2: 107–08). Croatia and Serbia

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are on either side of the borderline between the two Europes, which have always existed and which became more evident in the eleventh century by the consecration of the religious split between Catholicism and Orthodoxy. Even in the eighteenth century Croatia seemed more distant than Russia to Ottoman Serbia, which belonged to the Orthodox world.

For the sake of Yugoslavia and the federalist idea, Croatians are looked upon with suspicion and kept in a semiclandestine condition in the West. This is why I appreciate even more Professor Prpic's good study of his emigrated people in America. The mere existence of this community overseas testifies for the moral strength of the Croatian nation. The author notes that in the first years of the present century Ante Tresić-Pavičić, who visited America in 1904, wrote that in fifty years there would be no trace of Croatian nationality left in America because the grandsons of the immigrants would not speak a single Croatian word. They would not even know which nationality they descended from. This study amply shows that he was wrong

The exact number of Croatians living in the United States is unknown because United States censuses of population ignore them as a separate ethnic group and only indicate Americans whose country of origin is Yugoslavia. According to the census of 1960 there were 448,503 Americans of Yugoslav origin. The author thinks with Većeslav Holjevac that at least sixty per cent of these are of Croatian nationality and that the total count of Croatian emigration throughout the world is presently around 1,500,000—900,000 of which would be living permanently or temporarily in the United States and 50,000 in Canada. But no doubt all do not agree with his contention that the Croats in America are "members of an egalitarian society," as the domination of Americans of Anglo-Saxon descendants has been pointed out by several authors.

DIMITRI KITSIKIS
*University of Ottawa*

JOHN TOMSICH. *A Genteel Endeavor: American Culture and Politics in the Gilded Age.* Stanford: Stanford University Press. 1971. Pp. vi, 236. $8.50.

This graceful monograph examines the art and ideas of eight American Victorians whose literary success was almost as complete as their later oblivion: Richard Henry Stoddard, Thomas Bailey Aldridge, George Henry Boker, Bayard Taylor, George William Curtis, Charles Eliot Norton, Richard Watson Gilder, and Edmund Clarence Stedman. United by friendship and mutual admiration these figures were at the core of a literary establishment that controlled the best magazines, leading publishing houses, and the American Academy of Arts and Letters in the four decades following the Civil War. Defending culture, they praised each other, and as one friendly critic quipped, "policed Parnassus generally." This crusade provided ironic fulfillment of careers launched in rebellion against religious and social pieties in the antebellum years. No sign of complacency, their defense of beauty masked profound pessimism at the drift of events, an uneasiness that equally informed their social and economic writings. Judging their creative work derivative and sterile and their social prescriptions fear-ridden and rigid, Tomsich nonetheless concludes that their efforts substantially raised the level of American literature and scholarship. But even this mission, bereft as it was of a redeeming religious vision, seemed somewhat hollow to its partisans who could offer little resistance to mounting attacks. Success in a way was more painful than defeat since together they fostered a professionalization and bureaucratization in American letters that none wanted.

So viewed, these representatives of the "genteel tradition" fail to fit popular sterotypes in several regards. If excessive in self-esteem, they were not smug about their world. Their "literary Puritanism," a label Mencken applied, reflected neither personal prudishness nor religious conviction, but a more complex judgment concerning the nation's needs. Unlike Josiah Royce, whom Santayana judged typical of genteel culture, none were idealists in any formal sense. Devoted to "social order and harmony" they rejected Spencerian laissez faire no less than the Progressives whose vision of social responsibility they foreshadowed.

These conclusions are of course not entirely novel. These same figures have been rescued from Mencken in several recent biographies; and Tomsich agrees with Santayana more than

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he differs. Nor is this study entirely successful in relating the "genteel endeavor" to other movements, whether transcendentalism, realism, Spencerianism, or Progressivism. Although often suggestive, brief and sometimes superficial comparisons rest on external connections rather than internal analysis of ideas. More systematic analysis of magazine content and editorial practice might in addition have further tempered conventional characterizations of the "quality" magazines. Within the limits it sets, however, this intelligent, well-researched, and critically sympathetic study is a significant addition to a growing literature on society and culture in the Gilded Age.

ROBERT C. BANNISTER
*Swarthmore College*

O. GENE CLANTON. *Kansas Populism: Ideas and Men.* Lawrence: University Press of Kansas. 1969. Pp. xi, 330. $8.50.

BURTON J. WILLIAMS. *Senator John James Ingalls: Kansas' Iridescent Republican.* Lawrence: University Press of Kansas. 1972. Pp. ix, 201. $7.75.

WILLIAM WARREN ROGERS. *The One-Gallused Rebellion: Agrarianism in Alabama, 1865–1896.* Baton Rouge: Louisiana State University Press. 1970. Pp. x, 354. $10.00.

The word "populist" has been increasingly bandied about in American politics in the 1970s, often loosely and inaccurately or in a manner unfair to the genuine article of the 1890s. Despite this popular fuzziness, which only seems to increase as time passes, impressive scholarly studies of the agrarian revolt continue to appear with remarkable regularity. *Kansas Populism: Ideas and Men* is a detailed, chronological examination of Populism in its Midwestern center. Although it incidentally substantiates the work of Walter T. K. Nugent in absolving the agrarians from the charges of nativism and anti-Semitism, Clanton's book is both broader in its scope and more conventional as a history of Kansas Populism, with special attention to its leadership. The composite picture resulting from the biographical study of eighty-nine major leaders is interesting even if unsurprising: forty-six years old in 1890, this "typical" Populist leader came from the "rural middle class" and had graduated from college; he or she had been active in reform work for some time before 1890 and, although often alienated from the established churches, appraised society and its problems from the standpoint of Christian ethics. In short, Clanton concludes from his evidence that the leaders of Kansas Populism, rather than being "clodhoppers" or wild-eyed demagogues, "were, on the whole, an extraordinary group of individuals, iconoclastic in their appraisal of society, bold and at times radical in their solutions."

The Populist leaders may have been extraordinary, but John J. Ingalls, the Republican senator from Kansas from 1873 until the Populists ousted him in 1891, comes through in the biography by Burton J. Williams as a sharp-tongued mediocrity. If Williams's somewhat pedestrian account is correct, then surely the Kansas agrarians did themselves honor by replacing Ingalls in the Senate with William A. Peffer. Rarely advocating any significant or constructive legislation, according to his biographer, Ingalls seems to have established his best claim to fame by his bold statement, so aptly characterizing one aspect of the Gilded Age, that "the purification of politics is an iridescent dream. Government is force. Politics is a battle for supremacy. Parties are the armies. The Decalogue and the Golden Rule have no place in a political campaign. The object is success. To defeat the antagonist and expel the party in power is the purpose."

Williams suggests, somewhat half-heartedly, that Ingalls's Republicanism in the late 1850s had moral, antislavery dimensions and that as a postwar senator he habitually championed civil rights for Negroes. Yet Ingalls himself privately noted on one occasion that Senator Charles Sumner, whom Ingalls allegedly admired, had taken the floor of the Senate with "his negro bills, and various sentimental schemes for the amelioration of the condition of mankind." One can hardly help suspecting that for Ingalls, as for many other Republicans of his day, professed sympathy for blacks was but a patch on the "bloody shirt" that Ingalls waved prodigiously throughout his career.

Blacks and their civil rights are at least mentioned in the biography of Ingalls. They are not, interestingly enough, in Clanton's study of Kansas Populism; and that fact furnishes the most obvious clue to the difference between

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Populism in Kansas and in Alabama. In *The One-Gallused Rebellion* William Warren Rogers traces agrarian politics in Alabama from the end of the Civil War through the Bryan campaign of 1896. While Negroes and the race question are not in stage center, they are seldom completely off stage. Solid in its comprehensive, careful scholarship and conventional in its interpretations, Rogers's work supplies rich details for the agrarian story as originally told mainly by John Hicks and C. Vann Woodward. Having found the agrarian protest in Alabama "positive, rational, and realistic," Rogers differs in many of his conclusions from those offered by Sheldon Hackney in a recent book. But such historiographical aspects are played down by Rogers in favor of a straightforward narrative that is both analytical and readable.

ROBERT F. DURDEN
*Duke University*

WILLIAM M. DICK. *Labor and Socialism in America: The Gompers Era.* (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. 211. $10.95.

The failure of a mass socialist or labor party to emerge in the United States as it did in Europe is usually explained on the basis of one or more allegedly determining aspects of the American environment: immigration, or social mobility, or affluence, or property-consciousness, or what not.

William Dick re-examines the relationship between the trade union and socialist movements in the United States from the 1880s to the death of Samuel Gompers in 1924. In its first year of existence the AFL supported Henry George's campaign for mayor of New York City. Whereas in England the single-tax movement stimulated the development of socialist politics, in America George's defeat and the squabbles among his followers that ensued permanently disillusioned Gompers. Still in some sense a Marxist, perhaps even a socialist, Gompers became hostile to third-party politics. This inclination was reinforced by the efforts of Daniel De Leon, America's foremost socialist in the 1890s, to challenge the AFL through the Socialist Trades and Labor Alliance. In Dick's judgment, De Leon's "determination to control the trade unions or destroy them probably did more to alienate trade unionists from socialism than any other force."

After 1900 the Socialist party under Debs sought a fraternal relationship with the AFL in which each would have been an independent arm of a labor movement including both. By this time, however, what Dick charitably terms Gompers's "syndicalism" had hardened into dogma. Sensing that a "body of untapped support" existed among the unskilled workers which the AFL declined to organize, many socialists sought to reach these potential supporters by joining in the formation of the IWW, for Gompers a new version of the socialist dual unionism of the 1890s. Henceforth Gompers maintained a policy of rewarding labor's friends and punishing labor's enemies, which in practice signified an alliance with the Democratic party.

Dick concludes that "decisions taken by labor leaders on certain critical occasions determined this outcome." He finds Gompers's outlook more complex and less consistent than often assumed. While this judgment is based primarily on Gompers's letters and speeches, and Dick would have done well to give more attention to what Gompers did as well as said, his general line of argument deserves to be pursued. Were it to be extended into the 1930s, when the unskilled were finally organized but still without producing a mass party on the Left, one might find that John L. Lewis and the Communist party played a similar role in determining what failed to happen.

STAUGHTON LYND
*Chicago, Illinois*

WAYNE FLYNT. *Duncan Upshaw Fletcher: Dixie's Reluctant Progressive.* Tallahassee: Florida State University Press. 1971. Pp. ix, 213. $10.00.

FRED GREENBAUM. *Fighting Progressive: A Biography of Edward P. Costigan.* Washington: Public Affairs Press. 1971. Pp. v, 192. $7.00.

RICHARD LOWITT. *George W. Norris: The Persistence of a Progressive, 1913–1933.* Urbana: University of Illinois Press. 1971. Pp. xv, 590. $15.00.

LEROY ASHBY. *The Spearless Leader: Senator Borah and the Progressive Movement in the 1920's.* Urbana: University of Illinois Press. 1972. Pp. x, 325. $10.00.

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As long as historians viewed Progressivism as the interaction of political theories, biography was a logical means of approaching the subject. In the Olympian clashes of ideologies the natural tendency was to focus on the lives and thoughts of those articulate few who best exemplified the intellectual nature of the movement and whose opinions were accessible in numerous published writings and manuscript collections. Recent scholarship, however, has demonstrated that Progressivism was also the complex interaction of groups of men—individually perhaps of little historical note—like ethnic blocs, voluntary associations, trade unions, and even political clubhouses; that it was in the broadest sense politics as well as political theory. As historians continue to examine the institutional aspects of Progressivism, we may see fewer biographies. Meanwhile, they proliferate. The current crop, while varying in value, adds to our understanding of the actions, if not always the motivations, of individual Progressives. They also reconfirm the notion that Progressivism, far from being a single movement, is merely an umbrella term covering a messy array of programs and types.

It would be hard to imagine, for example, two men more fundamentally dissimilar than Duncan U. Fletcher and Edward P. Costigan. According to Wayne Flynt's lucid and well-written study, Fletcher's roots in the Reconstruction South left him with a lifelong suspicion of the uses of centralized governmental power, while at the same time his own efforts to rebuild his family's shattered fortunes made him sensitive to the values of "cooperation and social democracy." His leanings toward Progressivism were reinforced during the 1890s by fear of the growing strength of Populism. Seeking a more conservative vehicle of change, he aligned himself with the "Straight-Out" or reform wing of Florida's Democratic party. As a state legislator and mayor of Jacksonville he initiated many municipal reform programs, and, while he later acquiesced in the movement to disenfranchise Negroes, in his early career he built a local constituency of Negroes, white reformers, and labor. The conflict between Fletcher's natural conservatism and his social conscience might have reduced his effectiveness if he were not also endowed with a pragmatic instinct for political survival.

In his long career in the United States Senate (1908–36), he submerged his scruples against the use of federal authority whenever the economic welfare of Florida was at stake—as in his support of agricultural credits, a federally owned merchant fleet, and the development of inland waterways—but quickly retreated to a Constitutional defense of states' rights on such matters as the child labor amendment and the antilynching bill. Nevertheless Fletcher was an unswerving supporter of the New Deal, a fact that Flynt attributes as much to his humanity as to his party regularity. Despite Fletcher's differing motivations Flynt at every step valiantly attempts to explain the reasons for his actions. If the author occasionally seems to be reaching, or if his conclusions are sometimes tenuous, the fault is not his. Although his research was thorough he was handicapped by the fact that all of Fletcher's private papers were destroyed after his death by an overprotective secretary, an act of misguided devotion normally committed by the subject's widow.

Unlike the pragmatic Fletcher, Costigan was a man with a "single-minded devotion to political principle." In his defense of the many against the elite, he took his Jeffersonianism straight with just a dash of populistic fervor. Yet never was he afraid to use the state as a positive force to benefit the deprived. Unconcerned with party loyalty he ran the gamut from Republican to Progressive to Democrat all by 1916. Costigan's election to the Senate in 1930 (his only elective office) capped a long career in Colorado as civic reformer, labor attorney, and crusader for the better treatment of children. He was also the only member of the federal Tariff Commission during the 1920s who consistently fought for a scientific tariff structure. In the Senate he characteristically cosponsored a federal system of unemployment relief proposed by advanced social workers at a time when even most of his liberal colleagues opposed it. His finest hour, of course, was his courageous and solitary battle to pass the Wagner-Costigan antilynching bill, a battle he pressed until his health broke in 1936.

Costigan is an intriguing figure both because of the breadth and consistency of his Progressivism and because, unlike so many "old" Progressives, he made a comfortable transition to

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the New Deal. He seems contemporary to us in ways that Fletcher, Norris, and certainly Borah do not. But therein lies a trap that has ensnared Mr. Greenbaum, for it is one thing to admire Costigan's modernity, it is quite another to find in his liberalism, as Greenbaum does, a paradigm of nearly every present-day movement of reform or protest from Cesar Chavez's "Huelga" to open enrollment. A little presentism never hurt anyone, but when Greenbaum ponders Costigan's ghostly response to the army's recent purchase of grapes or concludes that "a staunch libertarian he warmed the embers for those who resist the oppressive tactics of the McCarthys and the Agnews," he has revealed a penchant for teleology that calls his work into question. Furthermore, the organization of the book (which lacks citations) is awkward and confusing, often causing us to lose sight of Costigan's role in the events described. The writing itself lacks the grace and flow that serve as a companion to comprehension. The author, for example, seems not to have discovered the personal pronoun (*vide* the surfeit of "La Follette's" on page 40), and sentences like "His argument for state control was narrowly locality oriented," are not uncommon enough. All this is unfortunate since Costigan is not likely to find a more genuinely sympathetic biographer than Mr. Greenbaum.

By almost any yardstick save that of physical stature, George W. Norris was a giant, certainly one of the two or three greatest senators in American history. With the publication of *The Persistence of A Progressive,* the second of a projected three volumes, Richard Lowitt is well on his way to writing a biography that should stand for many years as definitive. While the outlines of Norris's story are already well known, Lowitt's prodigious research and dedication to detail have produced a book that is at once a balanced personal portrait and a valuable reference tool for historians of Progressivism. Here we are offered many illuminating insights into the democratic and constitutional theories that governed Norris's irreconcilable opposition to the Versailles Treaty, his successful crusade to save Muscle Shoals, and his seemingly hopeless attempt to use federal power as a "countervailing balance" on behalf of workers and consumers. In the past, works about Norris have focused on his thought, the life of his mind—understandable enough considering that he was one of the leading Progressive theorists in public life. But as a result the Nebraska senator has been made to seem more like an institution than a person. Lowitt reminds us that he was also a consummate politician, a brilliant legislative tactician, a loving family man, and a witty—though never malicious—antagonist. Believing that the achievement of lasting reform was dependent on institutional changes, Norris bent his parliamentary skills to making Congress more responsive, fought a decade-long battle against the anachronistic short session (resulting in passage of the "Lame Duck" amendment), and championed a socially conscious Supreme Court. A writer once said that "sooner or later almost every one . . . who speaks . . . for the underdog drifts into Norris' office." Henceforth those seeking to know about Norris will have to pay Mr. Lowitt the same courtesy.

Finally there is Borah who, despite all that has been written about him, remains something of an enigma—part Progressive, part curmudgeon, and part prima donna. Mr. Ashby's treatment of the "spearless leader" is intimately related to his view of the course Progressivism took during the 1920s. He accepts the thesis that the prewar movement, unified by a "reform ethos," broke up in the twenties into competing special interest factions, the most important effect of which was to emphasize the deep "cultural antagonisms" between rural and urban values. Essentially an atomist, Borah sought at most a "balance among various groups," which would allow each to work out its own "place in the sun." The growing needs of the cities made "special" demands on the Progressive conscience, demands that men like Borah were ill-equipped to handle. As his political effectiveness waned he turned increasingly to pietistic moralizing about the public (as opposed to special) interest, the sanctity of the Constitution, and the outlawry of war. At the same time, he drew closer to the Republican establishment, and even on the all-important issue of farm relief he never strayed far from the official policies of Harding and Coolidge. Borah's failure to re-form a Progressive coalition in the late 1920s is therefore not really surprising. He was already a man whose time had past, a lonely high priest lighting votive candles to outdated gods.

Mr. Ashby, whose book is eclectic in the best

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sense, reveals throughout an impressive familiarity with the historiography of the Progressive movement—from Hofstadter to Otis Graham—and his analysis of Borah's personality, his strengths, his weaknesses, and his ultimate failure, is, if not unique, cogently argued and imaginatively organized. Ashby writes with vitality, color, and a keen eye for the effective quotation.

We end, however, where we began, wondering if biographies of men like Borah, valuable as they are as personal studies, can help us get at the heart of the Progressive movement, particularly in the 1920s. Borah typifies the problem. He was a Republican, a rural-type Westerner, and a United States senator. Yet the Progressive main line now ran through the cities, mainly in the East. Its skirmishes were fought chiefly on the local and state levels—and more often than historians have acknowledged, within the Democratic party—by social workers, ethnic political leaders, and associations of anonymous men and women. Its leaders saw, as Borah and his ilk did not, that special interests are not all equally dangerous, that "class" legislation for labor and the poor merely constituted compensatory action to redress former neglect. When the New Deal arrived, they were there to greet it. It is with these people, I suspect, that we shall find the key to Progressivism in the 1920s.

PHILIP DE VENCENTES
*Dictionary of American Biography*

HAZEL W. HERTZBERG. *The Search for an American Indian Identity: Modern Pan-Indian Movements.* [Syracuse:] Syracuse University Press. 1971. Pp. ix, 362. $12.00.

Every few decades that perennially popular topic, the American Indian, produces a bumper crop of publications. The most recent period of abundance set in about 1968 and will probably continue for several more years. Meanwhile, catering to faddish reader tastes is already apparent in multiple treatments of overworked themes: the put-upon Indian, the Indian as environmentalist, the Indian as fictional hero, and so on. Hazel W. Hertzberg's study of modern pan-Indian movements will doubtless be among the few books to have enduring scholarly value from the current era of Indian awareness. Her analyses will be criticized and modified, and new data will be brought to bear regarding the topic, but she has opened our eyes to a neglected subject and has done an excellent job in bringing new information and sources of information to our attention. Her work begins to fill the gap between our knowledge of intertribal confederacies from contact times to the later nineteenth century and studies of contemporary Indian "militancy" and "red power" organizations. She concentrates particularly on the rise and demise of the first modern Indian reform movement of truly national scope, the Society of American Indians (SAI), founded in 1912. Developing a useful taxonomic scheme Hertzberg distinguishes such reform pan-Indianism from religious and fraternal pan-Indianism. It is an enormous topic, and Hertzberg wisely gives less extensive treatment to fraternal and religious movements, merely supplying enough substantive contrast to put the reform movements into sharper focus and to suggest fascinating areas for further research on the various kinds of pan-Indianism. Future studies will doubtless note additional categories and refinements of those Hertzberg defines.

We need to know more, for example, about the function of the powwow circuit in promoting pan-Indianism, the relationships between pan-Indian and intertribal approaches in developing Indian unity to pursue Indian sociopolitical goals, and the role of Indian-run news media rather than formal organizations in rallying Indian support on given issues. Furthermore, while Hertzberg makes a convincing case that pan-Indianism of the pre–World War I era was greatly influenced by organizational models from white society as well as by white values, she does not analyze the inadequacy of analogies drawn between Indians and immigrants as possibly accounting for the failure of the SAI to serve the needs and aspirations of the mass of Indian people.

The most valuable feature of the book is what obviously interests Hertzberg most deeply, the historian's sheer delight in tracking down the personalities, activities, and accomplishments involved in a heretofore unexplored topic. Her account of the SAI is a major contribution to the literature on the American Indian. Although Hertzberg writes well, it should be noted that this is not a book for the

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ordinary layman. It will be of greatest value to the reader who has some knowledge of Indian policy, tribal cultures, American political history, and the relationship between anthropology and Indian people. It should also be useful to modern Indian activists in giving a broader perspective and depth of understanding to their concerns and efforts.

NANCY OESTREICH LURIE
*Milwaukee Public Museum*

HENRY CLEPPER. *Professional Forestry in the United States*. Baltimore: Johns Hopkins Press, for Resources for the Future. 1971. Pp. ix, 337. $10.00.

*Professional Forestry in the United States* is a readable account of a forester's view of his history. Not a newcomer to the history of forestry and conservation, Henry Clepper was editor of *Origins of American Conservation* (1966), *Leaders of American Conservation* (1971), and coeditor of *American Forestry: Six Decades of Growth* (1960). His forestry career began in 1921, and from 1937 until his retirement in 1966 he was executive secretary of the Society of American Foresters. This close association with the development of forestry in America has given him insights unavailable to other authors.

From the beginning, foresters joined with the lumbermen to solve their mutual problem of how to log profitably and still protect the basic forest resource from permanent depletion. Through education, subsidy, incentive, and reforms they worked to eliminate fire, obtain equitable forest-land tax schedules, and assure future timber supplies. The success of their team effort can be measured in terms of the Weeks Law of 1911, the Clarke-McNary Law of 1924, the growth of the U.S. Forest Service and state forestry agencies, revised tax codes, and the industrial tree farm movement.

Clepper uses forestry's sacred trilogy—fire, taxes, and scarcity—to explain all aspects of the growth of American forestry. To him, scarcity, the timber famine, was a two-edged sword; it gave impetus to the conservation movement of Theodore Roosevelt and Gifford Pinchot, while, antithetically, prompting lumbermen to stockpile forest land to offset impending exhaustion. Forest fires and rising property taxes threatened these timber stockpiles, and lumbermen began to cut faster in order to minimize anticipated losses. Rapid cutting laid waste to even more land and glutted the market with lumber, forcing prices ever lower, which necessitated cutting and selling still more and more.

That the lumberman wanted and needed the forester's help out of a vicious economic crossfire is a neat argument and intuitively acceptable. It is a view that foresters do not question, but historians should. If closeness to the subject provided Clepper with insights denied others, it also made him unable to objectively examine the major issues of forestry's history in America.

Clepper tells us much about fire, taxes, and scarcity, but he does not challenge the validity of the belief in their fundamental roles. He does not examine timber inventories, such as those of Henry Gannett and the Forest Service, or market conditions to see whether or not physical or economic scarcity existed or even threatened. We see no statistics that measure the real danger from fire, nor are we made aware of criticism by Ashley Schiff and many conservationists of forestry's obsession with fire prevention. And we are not shown that two major studies by Fred R. Fairchild refuted the basic contention that rising or unfair property taxes forced overcutting on a significant scale.

Despite this criticism Clepper has described in detail the sacred trilogy that was indeed uppermost in the minds of foresters and lumbermen as they charted the development of American forestry. In doing so, he provides a valuable service; but a history that rigorously analyzes forestry's growth still remains unwritten.

HAROLD K. STEEN
*Forest History Society*

CHARLES CHATFIELD. *For Peace and Justice: Pacifism in America, 1914–1941*. Knoxville: University of Tennessee Press. 1971. Pp. viii, 447. $11.95.

The last five years have seen the publication of several good books about the history of pacifism in the United States, and this excellent work by Charles Chatfield of Wittenberg University, who is president of the Conference for Peace Research, may be regarded as the standard work for its period. Building upon Merle

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Curti's pioneering work of three to four decades ago, contemporary historians of pacifism have produced a large and splendid body of work.

All kinds of people in the peace movement—from radicals to whom pacifism was only a part of a sweeping attack on social injustice, to people affiliated with the historic peace churches, to relatively conservative opponents of war—failed in their intense efforts to keep the United States out of World War I, and they suffered defeat after defeat from 1917 until about 1920. Many of them underwent terrible personal hardship during those repressive years as well. From then until the late 1930s the peace movement reached its greatest strength, succeeding in the 1930s in crystallizing what had been an inchoate antiwar sentiment and, working effectively with neutralists and isolationists, bringing its point of view into domestic politics in a major way. The movement lost ground steadily after the outbreak of the European war in 1939; pacifist effectiveness had largely disappeared well before Pearl Harbor.

The author devotes some space in this book to the organizational history of the many peace organizations. (There were so many of them that the reader may occasionally become confused despite the clarity of Chatfield's writing.) For the most part, however, the main focus of this book is on the influence of the peace movement as a whole upon the rest of American society and on the way that national and international forces and events affected the pacifist movement. For all his attention to the sometimes frantic activities of the pacifist organizations, however, Chatfield clearly agrees with Lord Keynes's statement that "soon or late, it is ideas, not vested interests, which are dangerous for good or evil." He puts major emphasis upon pacifist ideas, intellectual assumptions, and moral and religious principles. Not only does he treat with understanding such well-known figures as Reinhold Niebuhr and A. J. Muste, he discusses the thought of lesser-known pacifist thinkers. The too-much-neglected Richard Gregg is an outstanding example. It was largely Gregg who introduced Americans to the philosophy of nonviolence of Mohandas K. Gandhi, and Gregg's 1934 work, *The Power of Nonviolence,* had an important influence here and abroad that transcended the peace movement.

This is an important and rewarding work. It is well researched, clearly—if a little heavily—written, observant, and thoughtful. Of obvious relevance for those interested in the peace movement, past and present, it also merits reading by those whose interests lie in American social thought and in the domestic political background of the nation's involvement in the two world wars.

DAVID A. SHANNON
*University of Virginia*

GEORGE T. BLAKEY. *Historians on the Homefront: American Propagandists for the Great War.* [Lexington:] University Press of Kentucky. 1970. Pp. 168. $7.00.

WILLIAM SUMMERSCALES. *Affirmation and Dissent: Columbia's Response to the Crisis of World War I.* (Teachers College Studies in Education.) New York: Teachers College Press, Teachers College, Columbia University. 1970. Pp. xii, 159. $7.25.

The last several years have witnessed a widespread growth of interest in war as an agent of historical change. Under the influence of the cold war and specifically of the Vietnamese conflict, increased attention has been focused on the consequences of war including its impact on intellectual life. Two recent books attempt to deal with this subject in relation to the First World War.

George T. Blakey's volume examines the varied activities of a significant group of American scholars on behalf of the Allied cause in World War I. United States intervention in 1917 brought forth several organizations that attempted to enlist members of the intellectual community into wartime service. Historians were especially active in the National Board for Historical Service, George Creel's Committee on Public Information, and, to a lesser extent, the National Security League. As the volume's subtitle suggests, these professional men found it difficult to maintain the standards of scholarship they had upheld for years. Many who had promoted the scientific method and dispassionate presentation abandoned all but a pretense of objectivity. Blakey shows how these historians drew false parallels between past and present in order to rally Americans in support

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of Wilson's crusade. Although not all historians engaged in these activities the author demonstrates that those who did were among the most eminent, including J. Franklin Jameson, Claude Van Tyne, Andrew McLaughlin, Albert Bushnell Hart, Dana C. Munro, Evarts B. Greene, Guy Stanton Ford, Waldo G. Leland, and James T. Shotwell.

The details are recounted with clarity, fairness, and keen analysis. Blakey minces no words in concluding that "history is easily distorted by passion" and in suggesting that historians will do well to avoid the temptation to wear two hats—"scholarship and political activism." His conclusions are well argued and worth pondering, and the study is a solid piece of research based on the type of scholarship that far too many historians abandoned during the First World War.

William Summerscales's more narrowly conceived account of the impact of the Great War on a single university offers an interesting companion piece to Blakey's book. *Affirmation and Dissent* examines the relationship between the wartime crisis and educational problems at Columbia University. A somewhat breezy introduction is followed by detailed discussion of the presidency of Nicholas Murray Butler, faculty unrest, the dismissal of Professor J. McKeen Cattell, and curriculum development. In a key chapter, "From Antimilitarism to Intervention," the author explores a significant theme that is hardly touched upon by Blakey. By describing the prevalent hope that war would bring domestic and international renewal, Summerscales elucidates an important motive that impelled historians and other academicians to take up the cudgels for war. In other parts of the volume, however, inconclusive statements and digressions from the narrative occur. Although he writes on the whole with narrative skill and a felicitous style, his work is marred at times by a presentation that rambles haphazardly from one subject to another. This is particularly true in the first and final chapters. But despite these minor criticisms the author has provided a number of useful insights and produced an interesting contribution to the intellectual history of the First World War.

KEVIN J. O'KEEFE
*Eastern Illinois University*

RONALD C. TOBEY. *The American Ideology of National Science, 1919–1930.* [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. xiii, 263. $9.95.

Professor Tobey addresses himself to what he believes are the origins of the contemporary two cultures problem. He outlines a most provocative thesis and, by example, opens up neglected areas of inquiry by suggesting that scientists have ideologies and values—a welcome and notable contribution.

Tobey traces the unsuccessful attempts of a small coterie of "leading" American scientists—chiefly George Ellery Hale and Robert A. Millikan—to articulate an ideology of "national science" in the 1920s, the purpose of which was to persuade the public of the relevance of the values and methods of basic science to those of the larger American culture. Although Tobey's "national scientists" tried to popularize their ideology through various means, they failed because the new physics shattered the classic relevatory view of science upon which their ideology depended, the debate over Einstein's theories of relativity confused and alienated the public, and the collapse of prewar Progressivism vitiated their efforts to relate basic science values to the larger progressive cultural values. In the later 1920s Tobey's "national scientists" undertook to persuade big businessmen to support basic science, which had the effect of forcing these scientists "to relate their services to the long-term interest of gigantic corporations which were hardly concerned with the general welfare" (p. 230).

Tobey's thesis is bold and exciting; it deserves more comment than is possible here. Yet the overall focus and the approach of the book are too narrow. He ignores the biological and social sciences, the general cultural history of science, and professionalization, which is critical to the two cultures problem. He makes some questionable statements, not merely about the laws of thermodynamics, but about the history of the scientific community and its relationship with the public. He apparently believes the Einstein controversy is more important than the Scopes trial—which is interesting if true, but his account is unconvincing. And by stressing ideology Tobey misses the opportunity to investigate the broader cultural context of post-1900 American science. Quite possi-

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bly the two cultures problem came not so much from the failure of the Hale-Millikan campaign as from the "victories" of the post-1890 generation of scientific institution-builders and champions of academic professionalization in many disciplines—Franz Boas, E. B. Titchener, T. H. Morgan, and others—who were the first American scientists to devise a "successful" institutional means for distinguishing between university professionals and pre-academic amateurs. If so, this probably lessens the significance of the national science ideology. Nevertheless, Tobey deserves much praise for a stimulating book that should open new areas of inquiry in an underdeveloped field of history.

HAMILTON CRAVENS
*Iowa State University*

WALTER SMITH SCHOENBERGER. *Decision of Destiny*. Athens: Ohio University Press. 1969. Pp. viii, 330. $9.95.

AVERILL A. LIEBOW. *Encounter with Disaster: A Medical Diary of Hiroshima, 1945*. New York: W. W. Norton & Company. 1970. Pp. 209. $8.95.

While *Decision of Destiny* traces the history of American nuclear weapons to the point of air-drop on wartime Japan, *Encounter with Disaster* examines the effects at the receiving end in Hiroshima. Professor Schoenberger, a political scientist at the University of Maine, is interested in the complex factors that led to use of the atomic bombs. After examining early developments in nuclear energy research, the creation of the euphemistically named Manhattan Engineer District (MED), and questions of unconditional surrender, Soviet policy, and Franklin D. Roosevelt's role, the author devotes sixty per cent of the book to a balanced analysis of Harry S. Truman's interaction with domestic and international forces. Schoenberger has read very widely in the teeming literature; his own newest or most original sources are the Harrison-Bundy papers in the MED files, Office of Strategic Service intelligence studies from 1944–45, and personal correspondence with a dozen major participants. He has also consulted the personal papers of Grew, Hopkins, Leahy, Roosevelt, and Stimson and conducted an interview with General Leslie Groves. The result is a conscientiously documented, excessively detailed study that illuminates some dark recesses but necessarily relies heavily on speculation and offers little that is substantively novel. Although Groves's contribution looms large, few will be surprised to learn that the new American president was less the maker than "the instrument of decision." The best chapter deals with international politics, morality, and nuclear weapons. One of the more provocative points is that much important intelligence never reaches government leaders and thus negatively colors national decision making.

Averill Liebow is a pathologist, now on the faculty of the medical school at the University of California, San Diego, who was plucked from a medical unit on Saipan in 1945 to participate as a lieutenant-colonel on a joint commission for the investigation of the human and physical effects of the atomic bomb in Japan. Dr. Liebow's diary, prepared in shorthand at the end of each day, covers the period between September 1945 and January 1946, with supplementary information on the subsequent preparation of the six-volume final report in Washington. While some of the investigators proceeded to Nagasaki, Dr. Liebow worked in Hiroshima, which he reached in mid-October 1945, after weeks of complicated preparation with American and Japanese colleagues in Tokyo. In the Schoenberger book, reference is made to the American target planners' original *desiderata:* that the urban objectives for A-bombing combine maximum military and civilian destruction. Dr. Liebow translates these cold-blooded prescriptions into terms of overt agony. "Finally," he writes, "we saw the shocking and breathtaking sight of Hiroshima . . . devastated, cold—an ash." And of "the mask of Hiroshima," the dark nut-brown pigmentation of burned faces exposed in the open to the searing bomb, the physician laments: "The sight is horrible, since it involves some of the youngest and prettiest of the women." A skillful blend of clinical observation and compassionate sensitivity, complemented by numerous and unusual photographs, the Liebow book deserves a wide audience.

The "necessity" for the atomic bombings of 1945 is still arguable but, after revisiting stricken Hiroshima with Dr. Liebow, one comes away with the sad impression that little

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has been learned politically during the three decades that ensued. At one point in his book, Professor Schoenberger comments on American moralizing: "Since other states reflected different political cultures they frequently failed to measure up to United States ideals. . . . During war, the enemy, widely thought to be evil and deadly, must be stamped out as a political force." Could it not have been said of Hiroshima and Nagasaki, as well as of certain hamlets in Vietnam today, that "in order to save them, they had to be destroyed"?

ALVIN D. COOX
*California State University, San Diego*

GEORGE F. KENNAN. *Memoirs.* Volume 2, *1950–1963*. Boston: Atlantic—Little, Brown. 1972. Pp. viii, 368. $12.50.

Like its predecessor that I reviewed in this journal, this engrossing volume can be read as an "installment in the autobiography of an eminent historian; as the intellectual odyssey of a sensitive student of international relations; as an instructive portrait of a professional diplomat, alienated from society and impatient with domestic pressures; as a contribution to the historiography of the cold war; and as a commentary on decision making in recent American foreign policy" (*AHR*, 74 [1968–69]: 762). Because it covers a shorter time period in fewer pages and because Kennan held only two diplomatic posts in these years, the sequel presents less firsthand information on the shaping of United States actions abroad. More an observer than a participant, the author offers an intensely personal and often introspective account that, with its recurring themes of disappointment, frustration, estrangement, and even failure, recalls the autobiography of another philosophically minded historian, Henry Adams.

Of the twelve chapters, two deal with Kennan's initial stay, beginning in September 1950 as a Foreign Service officer on leave, at the Institute for Advanced Study and with his writings, lectures, and consultative work. Two others retrace, with the benefit of a longer perspective, issues touched upon in the first volume—the Korean War, the Japanese peace treaty, and the course to be pursued toward China and Indochina. These pages complement Dean Acheson's *Present at the Creation* (1969). The next three chapters illumine the shocking failure of the Truman administration to decide what it wanted Kennan to do when he arrived in Russia as ambassador in May 1952, the isolation and surveillance of the envoy in the last year of the Stalin regime, and the incredible chain of events that led to Kennan's ouster as a *persona non grata*. Then come two depressing chapters on the State Department's pusillanimity in 1952–53 when it lost the talents of not only Kennan but also such China experts as John Stewart Service and John Paton Davies. After an interlude reviewing the Reith Lectures for the BBC in 1957, with their criticism of American policy on Germany and nuclear weapons, Kennan concludes with his final assignment in Yugoslavia from 1961 to 1963. Here and in an epilogue he emphasizes and deplores the intrusion of domestic considerations in the formulation of foreign policy.

Like the first volume, this one draws upon a diary, unpublished reports, and other contemporary records. It offers glimpses of life in Princeton, Oxford, Moscow, and Belgrade. It comments on the case of Alger Hiss. It contains appreciative vignettes of J. Robert Oppenheimer, Edward Mead Earle, Josip Broz Tito, and John F. Kennedy. Given Kennan's dissent from their policies, Acheson and John Foster Dulles come off less well, although the former is treated more gently than he might have been if he had not died while the book was being written. Interestingly, after his final retirement from government and only four weeks before the tragedy at Dallas the author wrote Kennedy —who is now regarded in some quarters as the supreme cold warrior—that Kennan greatly admired the manner in which the president had addressed himself to problems of foreign policy and that Kennan had not seen "a better standard of statesmanship in the White House in the present century" (p. 317). But the most revealing portrait of all is a self-portrait, as the author mercilessly analyzes his own character and admits to his own shortcomings. In sum, this volume and its predecessor form one of the outstanding memoirs of our time.

RICHARD W. LEOPOLD
*Northwestern University*

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## LATIN AMERICA

A. OWEN ALDRIDGE, editor. *The Ibero-American Enlightenment.* Urbana: University of Illinois Press. 1971. Pp. x, 335. $10.00.

This book is the result of a conference on the Ibero-American Enlightenment held at the University of Illinois in 1969. Its contributions, by nine professors of literature, four historians, and one philosopher, are an attempt, in the editor's words, "to link the areas of North America, South America, and the Iberian Peninsula in an examination of the penetration of the thought of the Enlightenment" (p. vii). The essays are divided into sections on "Definition and Intellectual Background," "Iberian Peninsula," "Latin America," and "Anglo-America." Latin America, with six essays and eighty-seven pages, gets the most attention; Anglo-America, with two contributions, the least.

Within these broad categories there is considerable variation. Four articles are concerned primarily with individual eighteenth-century writers, two seek to examine literary movements connected with the Enlightenment, and two others have a geographical focus. The largest group of contributions and the editor's introduction take a broader approach, attempt to trace the general question of the impact of the Enlightenment, and seek to define it philosophically and historically.

The question of what role, if any, Iberia and her colonies had in the great intellectual changes of the eighteenth century is seemingly almost as old as the Enlightenment itself. Juan Marichal advances the suggestive idea that the Spanish Enlightenment "began . . . by not existing" (p. 97). One may add that it has had a hard struggle ever since, both to come into existence in fact and to secure acknowledgment of its actuality. Spanish conservatives have thought it a detestable eruption of heterodoxy, while partisans of the Enlightenment have considered the movement so late, attenuated, and derivative as to be negligible. When one adds the problems of limitation of expression and the difficulty of communication in Ibero-America, it is little wonder that many have concluded that the Englightenment scarcely existed. The latter point of view has been under modification for many years. Among Latin American historians, A. P. Whitaker and John Tate Lanning have probably contributed most to a more balanced view of the eighteenth century in Ibero-America.

One of the virtues of this book is that it makes clear that scholars are still puzzled about the exact nature and extent of the influence of the new ideas. Before one can discuss the influence of the Enlightenment one must define it, and differences of opinion are apparent almost at once. For example, in his introduction Aldridge adopts a definition of the Enlightenment of which the first and central point is acceptance of the discoveries of science even at the risk of displacing "intuitive or merely traditional beliefs" (p. 8). Peter Gay defines the same idea more succinctly as "hostility to Christianity." If hostility to Christianity is a key component of the Enlightenment, Ibero-America very obviously has little place in the movement. A. P. Whitaker on the other hand finds this proposition "too sweeping" and subject to too many major exceptions (pp. 47–48). Rather, Whitaker finds Latin America sharing many characteristics of the Enlightenment (for example, interest in practical reforms and improvements and a new style of patriotism) that are not necessarily anti-Christian.

The editor again very properly warns against excessive homogenization and congratulates Luis Monguió for pointing out that the colonial thought of the eighteenth century was restricted by orthodox belief. Yet Professor Monguió can find in this very fact a separation of theology from philosophy that he considers the "fulcrum of the Enlightenment" (p. 212). In short, the reader may find a very full indication of widely varying conclusions about the Enlightenment in Ibero-America and ample evidence that much thoughtful scholarship is yet required on this problem.

Singling out individual contributions in a work of this kind is apt to be an invidious task. Yet some of these essays are of more general interest than others. A. P. Whitaker's essay "Changing and Unchanging Interpretations of the Enlightenment in Spanish America" gives a thoughtful and judicious evaluation of the whole question. His review of current literature on the subject is most useful. The essays by Manoel Cardozo and E. Bradford Burns provide an illuminating perception of the too-little-known currents of intellectual change in

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Portugal and Brazil. Juan Marichal's "From Pistoia to Cádiz" examines the neglected and problematic connection between Spanish Jansenism and the Spanish Enlightenment. Russell Sebold provides a clear and suggestive treatment of the connection between Spanish literature and the new metaphysics of the Enlightenment. Luis Monguió makes a very interesting examination of the impact of the academic Enlightenment upon colonial periodical literature.

In summation, the contributors and the editor have indeed provided a most useful insight, not only into the continuing scholarship on the Ibero-American Enlightenment, but into the individual ways it is being defined and interpreted. Their conclusions are, for the most part, carefully sustained and clearly presented. It is a pity that the book was not indexed.

GEORGE M. ADDY
*Brigham Young University*

SHERBURNE F. COOK and WOODROW BORAH. *Essays in Population History: Mexico and the Caribbean*. Volume 1. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxiii, 455. $15.00.

This is a distinguished and monumental work that has been written by two of the nation's foremost authorities on the historical demography of Latin America. It presents seven new essays that build upon and yet substantially expand the depth of knowledge and the interpretation of data the authors have been probing for more than three decades. *Essays in Population History* is clearly the definitive work on the historical demography of Mexico, and when used in conjunction with earlier publications of the authors, many of which are frequently cited, the reader will have the benefit of as full a guide as sources now permit to comprehending the changes in Mexican demographic patterns in the colonial period and beyond.

Chapter 1 ("Materials for the Demographic History of Mexico, 1500–1960") is the most authoritative discussion to be found anywhere on the nature, merits, and frequently the location of the scattered and complex sources available to historians for the reconstruction of Mexican demographic history. Many of these sources are important for other than demographic studies; in fact, almost any Latin American historian, particularly those interested in the colonial period, would profit from reading this essay. Starting with the pre-Conquest period the authors discuss such sources as native traditions and written records, Spanish reporting on Indian society, and archeological and topographical data. In a long discussion of the colonial period, civil records (including fiscal materials, Indian tribute counts, assessments of non-Indians, *repartimiento* and *congregación* papers, *padrones generales*, and so on) and Church records (such as communicant counts, parish registers, tithe records, and so on) are detailed and analyzed. Concluding this important essay is a briefer look at the postcolonial period, in which civil and Church records of the republican period are discussed, including special mention of the accuracy and utility of vital statistics and national censuses.

Chapter 2 ("An Essay on Method") is a technical but necessary explanation of the complex (and frequently ingenious) methods, some of which are applied and introduced for the first time in this volume, used by the authors in retrieving, collating, and extrapolating data from extremely diverse and fragmentary sources. Despite such gaps, the authors report that "it is surprising how much of the data sought can be found in available reports from the past" (p. 73). Numerous concrete illustrations of the authors' methodology are provided.

Chapter 3 ("Family and Household in Mexican Enumerations since the Spanish Conquest") deals with the establishment of trends in numbers of inhabitants with respect both to time and locality. The chapter does much to clarify previous widespread confusion and disagreement over the meaning of the two key demographic entities, the family and the household.

Chapter 4 ("Civil Category and Age Group Ratios in Colonial Mexico") records changes in relative proportions of groups such as the civil categories of the married, the widowed, and the unmarried and age groups of infants, children, adults, and the old. Chapter 5 ("Population of West-Central Mexico: Nueva Galicia and Adjacent New Spain, 1548–1960") "explores methods whereby the movement of population in a moderately large area can be re-

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constructed over a substantial period" (p. 300).

Chapters 6 ("The Aboriginal Population of Hispaniola") and 7 ("The Historical Demography of Interior Tribes of Colombia . . .") depart from the authors' previous emphasis on Mexico. On the basis of a critical examination of the earliest European texts, the authors estimate the most likely range of population for Hispaniola at the time of the first European contacts as being between seven and eight million persons—a figure much higher than previous estimates, including the figure of three to four million set by Las Casas. Chapter 7 deals with the relation between climate and the extent of destruction of population in Colombia and is derived in large part from studies of Juan Friede and Germán Colmenares.

In order for a reader to derive maximum benefit from this remarkable work, careful and sustained attention is required, as is also a considerable background in the history of the regions studied and in the techniques and methods of demographic research and an acquaintance with the authors' previous writings. A second volume of *Essays in Population History*, which will deal with Yucatan and analyze racial mixture, reproductivity, and changes in the causes of death, will be published in 1973. Volume 1 is clearly and quite properly written for specialists rather than general readers, but is a thoroughly professional piece of research and writing, and the more than one hundred tables and figures add substantially to the merits of this outstanding contribution.

DONALD B. COOPER
*Ohio State University*

JOHN RUTHERFORD. *Mexican Society during the Revolution: A Literary Approach*. New York: Oxford University Press. 1971. Pp. x, 347. $13.00.

This book purports to be an innovative attempt to put into practice "the literary approach to historical sociology" by using contemporary novels as a principal source of information about Mexican society during the turbulent years from 1910 to 1917. The author in reality pursues three different and sometimes conflicting objectives. The most successful of the three is the substantive use of novelistic evidence to provide insights into social types and classes, preceded by an interesting discussion of the elusive "novel of the Revolution." Out of the 150 that fall loosely into this category he selects 30, published mostly between 1910 and 1925, to form the basis of his study. The novels portray ineffectual and disoriented intellectuals, degenerate *hacendados, la high life* of the Mexico City elite, the tawdry existence of petty bureaucrats, and the discreet activities of the *corredoras de alhajas* (jewel brokers) who sprang up like mushrooms to take advantage of rapidly changing fortunes. When Rutherford finds novelistic evidence disappointing, he turns to other sources to fill out his portrait of groups he divides into "revolutionary" and "anti-revolutionary." His treatment of legends that grew up around the various chieftains is especially effective, though based mostly on non-novelistic sources.

The author's second and less successful objective is methodological. He argues for the use of the novel as a "source of factual information" by the skilled literary analyst who can correct distortions of reality that may arise from the novelist's imagination. The argument, pursued at length in the opening chapter and continually thereafter, is confusing and it distracts the reader from the substantive strength of the book. Rutherford distinguishes novels from "orthodox sources" without ever defining the latter. When he refers to the "writer of allegedly factual accounts of human affairs" or to the "historian" (as opposed to the "novelist"), one is never sure whether he means the contemporary observer of events or the later analyst. His main point that historians should use novels more creatively is well taken, but his insistence on their "factual" content reveals an unfamiliarity with the now standard canons of historical criticism and subtle use of evidence, as outlined for example in Marc Bloch's handbook. Historians regard "facts" differently from the way they did fifty years ago, contrary to Rutherford's implications.

The third and least successful of the book's objectives is to contribute to revolutionary theory. The author concludes that the Mexican Revolution does not fit existing theories and that it might have been avoided, except for the weakness of the army. "But then," he adds in a typical ambivalent note, "the ultimate proof that it was necessary was that it did happen." Within this "theoretical" framework his treat-

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ment of specific revolutionary groups tends to be programmatic and judgmental. He tells us that "the intellectuals failed lamentably to carry out the duties the Revolution demanded of them," or that no "progressive sentiments" were expressed in the "sad love songs" sung by the fighting masses. These assertions, of which examples abound in the book, are more appropriate to the ideologist than to the scholar. Despite its substantive merits, the quality of the study suffers from its lack of methodological focus and its overly ambitious interdisciplinary approach.

CHARLES A. HALE
*University of Iowa*

HÉLIO SILVA. *1938: Terrorismo em Campo Verde.* (O Ciclo de Vargas, Volume 10. Documentos da história contemporânea, volume 11, part 1.) [Rio de Janeiro:] Civilização Brasileira. 1971. Pp. 421.

In 1938 a plot against the Brazilian government by some of Plínio Salgado's "Green Shirts" and other Brazilians unhappy with President Getúlio Vargas resulted in an unsuccessful and bizarre *putsch*. The events, sometimes grim, sometimes ludicrous, occurred in a cloak-and-dagger atmosphere and make a fascinating story, thanks to the pen and research of Hélio Silva.

For a while the reader finds himself in the role of a detective with the task of unraveling conflicting testimony about the attack that Severo Fournier led against the presidential palace. When the task seems most difficult the author steps in to clear things up. Convincingly he maintains that Fournier deserted his men after a short interval and before any legalist soldiers reached the scene—consequently Silva presents a picture of Fournier that differs from the one given in David Nasser's *A Revolução dos Covardes,* first published in 1947. Hélio Silva also finds it necessary to contradict Plínio Salgado's statement, made in 1953, that he "learned of the events only on the very night of the rebellion" of 1938.

Fournier's subsequent asylum in the Italian embassy and other matters related to the palace attack, such as Flôres da Cunha's anti-Vargas conspiracy in Uruguay, are woven into a broader story about some of the international complications that occurred when the world was approaching war and the Vargas regime was seeking to Brazilianize the country's foreign immigrants. Interesting letters from Italy, written after Brazil declared German Ambassador Karl Ritter *persona non grata,* tell of the efforts of Nazi diplomats in Rome to turn Italian leaders against Brazil.

To supplement information obtained from the Brazilian National Archives, newspapers, and books, Hélio Silva has consulted the files of Vargas, Eurico Gaspar Dutra, Filinto Müller, and Eduardo Gomes and has interviewed participants in the events. An index and a lengthy chronological table add to the usefulness of this enlightening volume.

JOHN W. F. DULLES
*University of Texas, Austin*

MARVIN GOLDWERT. *Democracy, Militarism, and Nationalism in Argentina, 1930–1966: An Interpretation.* (Latin American Monographs, Number 25. Institute of Latin American Studies, the University of Texas at Austin.) Austin: University of Texas Press for the Institute. 1972. Pp. xx, 253. $8.00.

The Latin American military as a subject for study continues to have a fascination for American scholars. In this volume Marvin Goldwert examines the role of the Argentine army from its first seizure of power in 1930 to its fifth in 1966 and offers a plausible explanation of its behavior. In his view, the army, as a result of a drive toward professionalization early in the century, became an autonomous institution outside civilian control. The breakdown of presidentialist government in 1930 opened the door to what Goldwert calls modern militarism. Thereafter the army operated as a power factor but was unable to bring social order and modernization to Argentina because of a fundamental contradiction in its own value system. Army officers are seen as nationalistic, anxious for a powerful military establishment based on industrial development but unwilling to pay the price in terms of the rise of new social forces and the erosion of traditional values. The effort to promote nationalistic modernization while preventing alteration of the social order is in Goldwert's view the explanation for the tortuous course taken by Argentine militarism and accounts, among other things, for ini-

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tial military support of Perón (until 1951) and the subsequent withdrawal of that support.

Goldwert's thesis tends to oversimplify the political behavior of Argentine army officers and to overstate the case for ideological commitment. For example, he posits two groups of army nationalists, liberal versus integral, who compete for control in the 1930s, with the latter group growing to predominance and seizing the state in 1943. Goldwert's insistence on regarding that coup as "an integral nationalist revolution" requires one to ignore the role that liberal, pro-Allied officers played in its success. Again his interpretation of the 1966 military seizure as a revival of the integral nationalist tradition overlooks the fact that the principal planner of the takeover was General Julio Alsogaray, whom Goldwert elsewhere labels a liberal nationalist.

Indeed, in these instances as elsewhere in the volume, the historical record as presented by the author is often debatable, for he has chosen to rely on journalistic or polemical accounts rather than explore all possible sources, primary and secondary. Interviews with a broad range of participants and not just United States newsmen in Buenos Aires might have produced data to challenge his assertions. So would a careful reading of various studies that have appeared in the last decade, including Juan V. Orona's five volumes on the military and Felix Luna's *El 45* (1969).

To sum up, Goldwert's book viewed as an interpretive essay is at once challenging and interesting; but the reader in search of a definitive account of the Argentine military's political role will not find it here.

ROBERT A. POTASH
*University of Massachusetts, Amherst*

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# Communications

*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

TO THE EDITOR:

A reviewer who takes other historians to task for misnaming an individual, as I did in my review of Robert Branyan and Lawrence Larsen, *The Eisenhower Administration* (*AHR*, 77 [1972]: 1182–83), has an obligation to keep the record straight himself. Unfortunately, I failed to do so. Eisenhower's first secretary of the treasury was not William Humphrey, who was actually the leading conservative on the Federal Trade Commission during the Coolidge-Hoover era, but George Humphrey, an Ohio industrialist who was one of the leading conservatives of the next era of Republican power.

ALONZO L. HAMBY
*Ohio University*

TO THE EDITOR:

I am grateful to Dr. Kinnear for his kind comments on my book *Politicians at War* (1971), and I trust that he will not mind the correction of two inaccuracies. He stated in his review (*AHR*, 77 [1972]: 521–22) that most of the book deals with the political crisis of May 1915. In fact, less than a third of the book is on May 1915. The longest and most original section, not mentioned by him, is on the British cabinet's decision for war in July and August of 1914.

According to Dr. Kinnear, I describe Professor Trevor Wilson's book *The Downfall of the Liberal Party* (1966) as " 'sparsely documented,' 'not persuasive,' and characterized by 'deductive gymnastics.' " Actually, the first of these phrases is applied not to Wilson's book as a whole, but to that part which deals with the years from 1914 to 1918. The other two phrases both refer specifically to *one* paragraph of Professor Wilson's book.

Dr. Kinnear is correct in saying that my book has no bibliography of published sources; he might, however, have added that there is a six and a half page list of manuscript collections consulted, including a number of important collections previously unused by scholars.

CAMERON HAZLEHURST
*Queen's College, Oxford*

MR. KINNEAR REPLIES:

I would like to clarify the points Dr. Hazlehurst raises about my favorable review of his book. His section on July–August 1914 is un-



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doubtedly original, but most readers will consider his meticulous and gripping dissection of the 1915 crisis to be far more interesting; and Dr. Hazlehurst leads one to consider 1915 as the core of his book by subtitling it *A Prologue to the Triumph of Lloyd George*.

I feel that the subject merits a good bibliography, particularly because Dr. Hazlehurst refers to a number of scarce, privately printed books in his footnotes. Perhaps he will include such a bibliography in his next book, *The Crisis of Liberalism*.

His main point concerns his views of Wilson's book. While Dr. Hazlehurst does not condemn Wilson's work *in toto*, he is certainly much more favorable to Lloyd George than Wilson was. The "one paragraph" to which Hazlehurst refers deals with Wilson's interpretation of Lloyd George's motives in 1915. This point is vital to both Wilson's and Hazlehurst's views of the subject. Indeed, one feature of Dr. Hazlehurst's book is that it comes squarely to grips with other recent academic writings on the period; this is, I think, of considerable interest to readers of this journal.

MICHAEL KINNEAR
*St. Antony's College, Oxford*

TO THE EDITOR:

Professor Paludan's article "The American Civil War Considered as a Crisis in Law and Order" (*AHR*, 77 [1972]: 1013–34) abundantly documents the expressed concern of Northern opinion for the maintenance of "government" in contrast to "anarchy" in the context of the North's decision to resist Southern secession in 1860–61. But words such as these may carry very broad overtones that the rather literal-minded Professor Paludan does not choose to explore. The thrust of his argument is that Northerners reasoned upward from their local governmental experiences to their concern for the survival of the national government. But others have long pointed to manifest destiny and continental empire building as prominent themes in the mid-nineteenth-century American imagination. Was there not a more grandiose level of nationalistic aspiration powerfully present in the Northern mind at this time, one expressed in the familiar slogan that "the Father of Waters must flow unvexed to the sea"? Wasn't the problem of secession instantly perceived by Northerners as an issue of national scope and consequences? Wasn't American greatness, the preservation of the territorial boundaries of the continental American empire, the issue that most deeply moved Northerners to "save the Union"? Is it therefore unlikely that "government" was here being used as a euphemism for "empire"? And so is there not something superfluous and a bit forced about Professor Paludan's explanation? National pride, the vision of a huge territorial unit welded by a patriotic sentiment that easily comprehends its entire physical extent in a single imaginative leap, is a factor that historians can ill afford to neglect if they are concerned to explain the eventual emergence of a world dominated by great power rivalries, a world in which American ambitions have scarcely played a minor role.

LAURENCE VEYSEY
*University of California, Santa Cruz*

PROFESSOR PALUDAN REPLIES:

It is difficult to argue with Professor Veysey. All the things that he suggests as possible parts of the Northern image of union may have existed. Indeed a reading of the literature on the meaning of union suggests that in fact they did. If Professor Veysey chooses to believe this body of writing rather than mine, that is his privilege. I suggest, however, that one view is not incompatible with the other; in fact I stated as much in my article. Surely the question of what made over a million people go to war is not to be responsibly answered by embracing single causes.

If I am too "literal minded" in my approach to suit him, my only response is to suggest that there is something to be gained by believing what people say about their own motives. This seems to me to be especially true when their daily experience accords so well with what they said. The people I described as responding to disunion by expressing their fear of anarchy may have been using government as a "euphemism for empire," but if they were they hid the fact well.

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Given the choice between rehearsing the old manifest destiny theme as a cause of war and exploring avenues not previously investigated seriously, I personally must choose the latter. If Professor Veysey wishes to analyze or reinterpret the existing ideas about manifest destiny, no one would welcome it more than myself. Indeed, as I am afraid my article did not make clear enough, I found his essay on American ideas about law and resistance most provocative and helpful. I am sure he could contribute much by a similar study of those ideas about union that intrigue him.

I did not intend that my article pre-empt all other explanations of the Northern decision to fight. I intended to suggest that historians have overlooked a perspective that deserves consideration. I think that what Professor Veysey objects to is not the fact that I read my sources too literally but that I did not accept literally the sources he prefers. I believe that the subject under discussion, the roots of loyalty, is complex enough that both of our preferences and many others besides should be explored if we truly want answers to why the war came.

PHILLIP S. PALUDAN
*University of Kansas*

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# Recent Deaths

HERBERT FEIS, the historian of recent American foreign policy, was born in New York City on June 7, 1893. He began his intellectual career as a Harvard economist, taking his A.B. in 1916 and his Ph.D. in 1921. After teaching economics at Harvard, Kansas, and Cincinnati in the twenties, he joined the Department of State in 1931. In the next dozen years, as economic adviser to the department, he stood at the intersection of a whirl of bureaucratic and doctrinal tensions, which in later life he would recall with amused incredulity and about which he wrote with clinical detachment. After the war his training as an economist and his experience as a public servant found full expression when he undertook a new career as a historian.

Beginning rather diffidently in 1947 with *Seen From E. A.*, Herbert Feis in the following quarter century wrote eleven books that together constitute a semiconsecutive history of American foreign policy from 1933 to 1950. These volumes, recording the extraordinary years in which the United States abandoned political isolationism and pursued, at first cautiously and selectively, then more recklessly, a course of global intervention, were published in a variety of formats and by a variety of houses. It is to be hoped that an enterprising publisher will eventually reissue them in a single uniform edition.

There has been discussion within the profession about the value of the participant historian. Though Herbert Feis did not reject the historical and intellectual framework within which American policy developed, he was by no means a writer of old-school-tie history, intent on proving the infallibility of official judgment. Rather he was always determined to seek out new documents, conduct new interviews, and reopen old issues. His work demonstrated, I think, not only that participation in events does not foreclose the possibility of reasonably dispassionate historical analysis but that it may enhance the accuracy and fullness of historical reconstruction.

Historians tend to be captives of their documentation; Feis, who was there, knew the limitations of documents. Historians, seeking patterns of intelligibility, often impute more coherence and rationality to decisions than were perhaps to be found in the consciousness of the decision makers. Feis had been through the mill, and there had burned into his soul the acrid knowledge of the curious and untidy way decisions were made. And he well understood the relativity of historical interpretation. "All historical tales," he wrote, "are tinted by the light of the times in which they are written. . . . I leave to the hour of more confident judgment the argument over what was done right and what was done wrong, who is to be praised and who is to be blamed."

His particular gift, demonstrated especially in such works as *The China Tangle* (1953) and *Roosevelt-Churchill-Stalin* (1957), lay in isolating and unraveling knotted strands of policy and personality. His historical *oeuvre* drew its power not only from his careful and wide-ranging research and his lucid skill in disentangling highly confused situations but from his ability to identify critical questions, his vigorous willingness to confront them, and his subtle perception of atmosphere and motive.



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Its final distinction came perhaps from the way that these qualities were ordered and absorbed in his literary style. It is hard to conceive how a man reared as an economist and hardened as a bureaucrat could have preserved a style at once so arresting, so incisive, so sardonic, and so evocative.

Herbert Feis would be disappointed if we were to recall his concerns as a historian and not mention the crusade to which he devoted much energy in his later life—his effort to liberate "the shackled historian" from excessive government secrecy. His years in the State Department lent special weight and responsibility to his conviction that government could open documents to scholars at a much earlier point without damage to—even with benefit for—the national security.

He spent most of his last years at his farm in York, Maine, wintering at Antigua in the West Indies, and often passing through New York City on one or another research expedition. There he would be seen at luncheon at the Century Club, his head cocked in courteous but slightly ironic attention, his discourse pointed and elegant, his eyes sparkling, his face wise and alive under a cascade of snow-white hair. He radiated a certain gaiety, both personal and intellectual—all the more remarkable when one understood how much pain he endured in these last years. He had the manners of an older time, but he never lost his zest for the latest in human insight or human folly. His concern over the course of American foreign policy never flagged. No one felt more keenly both the stupidity and the shame of Vietnam. He died in Winter Park, Florida, on March 2, 1972.

ARTHUR SCHLESINGER, JR.
*City University of New York*

HERBERT WING, JR., former professor of history at Dickinson College, died at his home in Carlisle, Pennsylvania, on September 13, 1972. Professor Wing, who was born in Minneapolis, Minnesota, in 1889, graduated from Harvard in 1909 *magna cum laude* and Phi Beta Kappa after completing his course work in three years. He received the A.M. in 1911 from the University of Wisconsin and from 1913 to 1914 studied at the American School of Classical Studies in Athens, Greece. In 1915, the same year in which he received the Ph.D. from Wisconsin, he began a career at Dickinson College that was to last until 1960. He became professor of Greek in 1920 and professor of history and chairman of the department in 1940. During World War II he set up and directed the Army Aviation Cadet program at Dickinson. Professor Wing is survived by a son, Herbert Gilman Wing, two grandchildren, and a brother.

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# Association Notes

At the AHA's annual meeting in New Orleans, honorable mentions for the 1972 Watumull Prize were awarded to Richard G. Fox for *Kin, Clan, Raja, and Rule: State-Hinterland Relations in Preindustrial India* and to John Patrick Haithcox for *Communism and Nationalism in India: M. N. Roy and Comintern Policy, 1920–1939.*



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# Other Books Received

Books listed were received by the *AHR* between October 1 and December 1, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

## GENERAL

ANDERSON, BRYCE, *et al. Participation in Politics*. Ed. by GERAINT PARRY. [Totowa, N.J.:] Rowman and Littlefield. 1972. Pp. ix, 340. $14.50.

ARMSTRONG, HAMILTON FISH (ed.), with JAMES CHACE *et al. Fifty Years of* Foreign Affairs. New York: Praeger, for the Council on Foreign Relations. 1972. Pp. ix, 501. $12.50.

BENNETT, BARBARA, and AMSTER, LINDA (comps. and eds.). *Who Said What (And When and Where and How) in 1971*. Vol. 1 *(December–June 1971)*. [New York:] Quadrangle Books. 1972. Pp. vi, 309. $12.50.

CLARK, DON. *Wild Blue Yonder: An Air Epic*. Seattle: Superior Publishing Co. 1972. Pp. 172. $12.95.

COBBAN, ALFRED. *The Nation State and National Self-Determination*. The Fontana Library. Rev. ed.; [London:] Collins; distrib. by New Viewpoints, New York. 1969. Pp. 318. $1.50.

DICKENS, A. G. *The Age of Humanism and Reformation: Europe in the Fourteenth, Fifteenth, and Sixteenth Centuries*. Englewood Cliffs, N.J.: Prentice-Hall. 1972. Pp. ix, 290. Cloth $8.95, paper $4.95.

ELLIOT, GIL. *Twentieth Century Book of the Dead*. New York: Charles Scribner's Sons. 1972. Pp. vii, 241. $7.95.

FENNO, RICHARD F., JR. (ed. and with an introd.). *The Yalta Conference*. Problems in American Civilization. 2d ed.; Lexington, Mass.: D. C. Heath. 1972. Pp. xiv, 218. $2.50.

FORMAN, JAMES D. *Capitalism: Economic Individualism to Today's Welfare State*. Studies in Contemporary Politics. New York: Franklin Watts. 1972. Pp. viii, 134. $5.95. Grades 7–12.

FORMAN, JAMES D. *Communism: From Marx's* Manifesto *to 20th-Century Reality*. Studies in Contemporary Politics. New York: Franklin Watts. 1972. Pp. viii, 150. $5.95. Grades 7–12.

FORMAN, JAMES D. *Socialism: Its Theoretical Roots and Present-Day Development*. Studies in Contemporary Politics. 1972. Pp. viii, 129. $5.95. Grades 7–12.

GEFFEN, WILLIAM (ed.). *Command and Commanders in Modern Warfare: The Proceedings of the Second Military History Symposium, U.S. Air Force Academy, 2–3 May 1968*. 2d ed.; [Washington:] Office of Air Force History, Headquarters USAF; [U.S. Air Force Academy, Colo.:] United States Air Force Academy. 1971. Pp. xi, 340. $1.50.

GOLDSMITH, EDWARD, *et al. Blueprint for Survival*. Boston: Houghton Mifflin. 1972. Pp. xiv, 189. $5.95.

GOLDSTON, ROBERT. *Pearl Harbor! December 7, 1941: The Road to Japanese Aggression in the Pacific*. World Focus Book. New York: Franklin Watts. 1972. Pp. 87. $3.95. Grades 7 up.

HACHEY, THOMAS E. *The Problem of Partition: Peril to World Peace*. Rand McNally European History Ser. Chicago: Rand McNally. 1972. Pp. xiv, 259. $3.50.

HALL, GUS. *Imperialism Today: An Evaluation of Major Issues and Events of Our Time*. New York: International Publishers. 1972. Pp. 384. Cloth $12.50, paper $3.95.

HUNTER, ROBERT. *Security in Europe*. 2d ed.; Bloomington: Indiana University Press. 1972. Pp. ix, 281. $8.95.

KEYSERLINGK, ROBERT WENDELIN. *Fathers of Europe: Patriots of Peace*. Montreal: Palm Publishers. 1972. Pp. viii, 175. $6.95.

KOCH, H. W. (ed.). *The Origins of the First World War: Great Power Rivalry and German War Aims*. New York: Taplinger. 1972. Pp. viii, 374. $3.95.

LE MOINE, ROGER (ed.). *L'Amérique et les poètes français de la Renaissance*. Collection Les isles fortunées, No. 1. Ottawa: Éditions de l'Université d'Ottawa. 1972. Pp. 350.

LOUIS, WM. ROGER (ed.). *The Origins of the Second World War: A. J. P. Taylor and His Critics*. Major Issues in History. New York: John Wiley



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and Sons. 1972. Pp. ix, 150. Cloth $6.95, paper $3.95.

MAŁOWIST, MARIAN. *Croissance et régression en Europe, XIV^e–XVII^e siècles: Recueil d'articles.* Cahiers des Annales, 34. Paris: Librairie Armand Colin. 1972. Pp. 223. 41 fr.

MARCUS, REBECCA B. *The First Book of Volcanoes & Earthquakes.* A First Book. Rev. ed.; New York: Franklin Watts. 1972. Pp. 86. $3.75. Grades 7 up.

MARSAK, LEONARD M. (ed.). *The Enlightenment.* Major Issues in History. New York: John Wiley and Sons. 1972. Pp. viii, 205. Cloth $6.95, paper $4.25.

MEEK, RONALD L. *Figuring Out Society.* [New York:] Fontana. 1971. Pp. 236. $1.75.

PANSINI, ANTHONY J. *Niccolo Machiavelli and the United States of America.* 500th Anniversary ed. Greenvale, N.Y.: Greenvale Press. 1969. Pp. vi, 1358. $20.00.

PARKER, W. H. *The Superpowers: The United States and the Soviet Union Compared.* New York: Halsted Press Division, John Wiley and Sons. 1972. Pp. xi, 347. $14.95.

REICH, WILHELM. *Sex-Pol: Essays, 1929–1934.* Ed. by LEE BAXANDALL. Introd. by BERTELL OLLMAN. Tr. by ANNA BOSTOCK *et al.* New York: Random House. 1972. Pp. xxviii, 378. $10.00.

ROBERTS, MARTIN. *Machines and Liberty, 1789–1914.* A Portrait of Europe, vol. 4. New York: Oxford University Press. 1972. Pp. 360. $5.50.

SIDDIQI, M. N. *Recent Theories of Profit: A Critical Examination.* [New York:] Asia Publishing House, for the Faculty of Social Sciences, Aligarh Muslim University. 1971. Pp. iii, 155.

SILBERSCHMIDT, MAX. *The United States and Europe: Rivals and Partners.* History of European Civilization Library. New York: Harcourt Brace Jovanovich. 1972. Pp. 216. $3.95.

SIMKINS, PETER. *Illustrated Book of World War II.* [New York:] St. Martin's Press. 1972. Pp. 128. $8.50.

SINGER, J. DAVID, and SMALL, MELVIN. *The Wages of War, 1816–1965: A Statistical Handbook.* New York: John Wiley and Sons. 1972. Pp. xii, 419. $13.95.

TAYLOR, CHARLES LEWIS, and HUDSON, MICHAEL C. *World Handbook of Political and Social Indicators.* 2d ed.; New Haven: Yale University Press. 1972. Pp. xiv, 443. $15.00.

WINKS, ROBIN W. (ed.). *Slavery: A Comparative Perspective. Readings on Slavery from Ancient Times to the Present.* New York: New York University Press. 1972. Pp. xvi, 217. Cloth $8.95, paper $3.95.

## ANCIENT

HEUER, KENNETH. *City of the Stargazers.* New York: Charles Scribner's Sons. 1972. Pp. 170. $7.95.

SPRAGUE, ROSAMOND KENT (ed.). *The Older Sophists.* A complete translation by several hands of the fragments in *Die Fragmente der Vorsokratiker,* edited by DIELS-KRANZ and published by Weidmann Verlag (Vaduz, Lichtenstein) (by whose permission the translations have been made), with a new edition of *Antiphon* and of *Euthydemus.* Columbia: University of South Carolina Press. 1972. Pp. x, 347. $7.95.

THOMAE, BENEDICTUS. *Laterculi Praesidum.* Vol. 2, *Tabulae Synchronae.* Pt. 1. Göteborg: Bokförlaget Radius. 1972. Pp. 60, xvi.

## MEDIEVAL

BALDWIN, JOHN W. *The Scholastic Culture of the Middle Ages, 1000–1300.* Civilization and Society: Studies in Social, Economic, and Cultural History. Lexington, Mass.: D. C. Heath. 1971. Pp. 124.

COPLESTON, F. C. *A History of Medieval Philosophy.* Torchbook Library Ed. New York: Harper and Row. 1972. Pp. 399. Cloth $11.00, paper $1.75.

HODGETT, GERALD A. J. *A Social and Economic History of Medieval Europe.* London: Methuen; distrib. by Barnes and Noble, New York. 1972. Pp. 246. $9.50.

LABAL, PAUL. *Le siècle de Saint Louis.* "Que sais-je?" No. 1481. Paris: Presses Universitaires de France. 1972. Pp. 126.

PRICE, MARY R., and HOWELL, MARGARET. *From Barbarism to Chivalry, 300–1300.* A Portrait of Europe, vol. 1. New York: Oxford University Press. 1972. Pp. 310. $5.50.

WILKINSON, BERTIE (ed.). *The Creation of Medieval Parliaments.* Major Issues in History. New York: John Wiley and Sons. 1972. Pp. xiii, 113. Cloth $6.95, paper $3.95.

WINTERBOTTOM, MICHAEL (ed.). *Three Lives of English Saints.* Toronto Medieval Latin Texts. Toronto: Pontifical Institute of Mediaeval Studies for the Centre for Medieval Studies. 1972. Pp. 94.

## BRITISH COMMONWEALTH AND IRELAND

BANTON, MICHAEL. *Racial Minorities.* Fontana Books. [London:] Collins; distrib. by New Viewpoints, New York. 1972. Pp. 192. $1.95.

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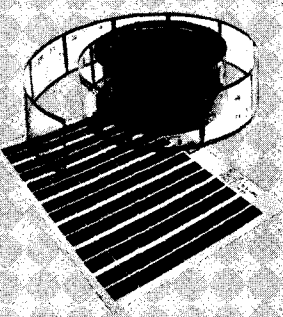


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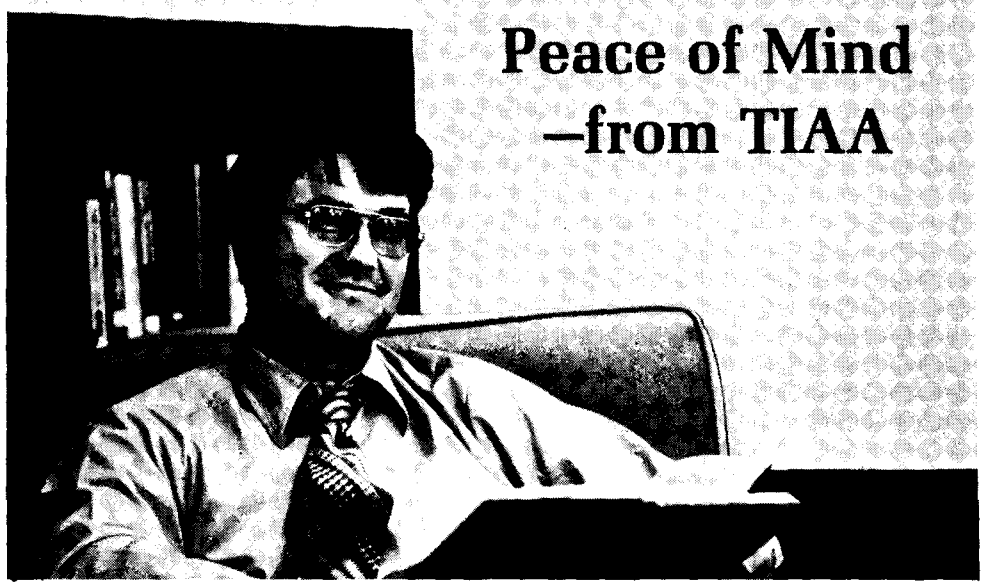
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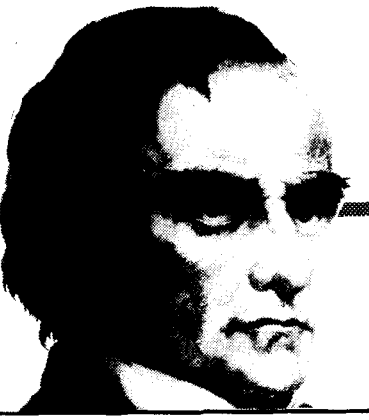
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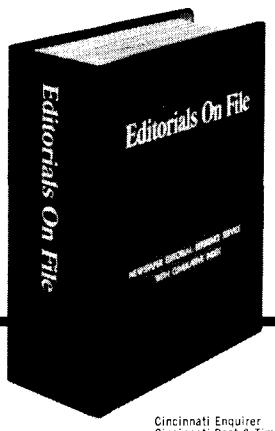
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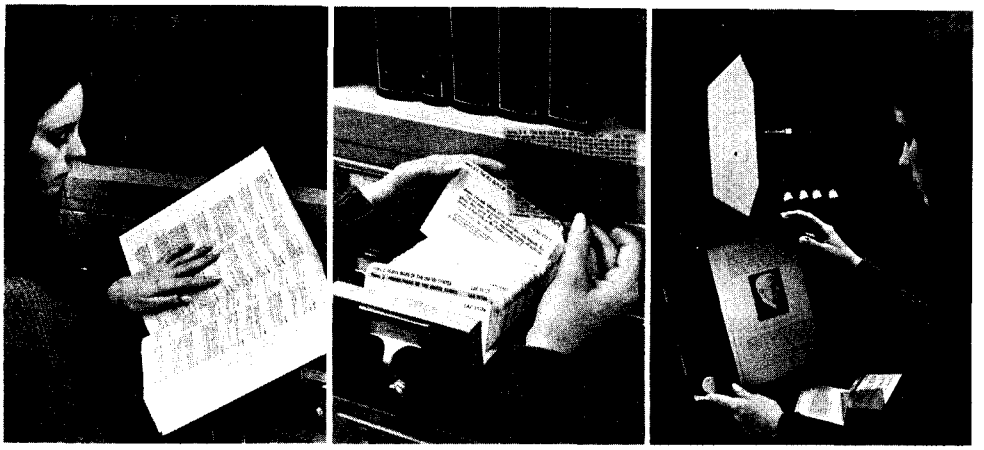
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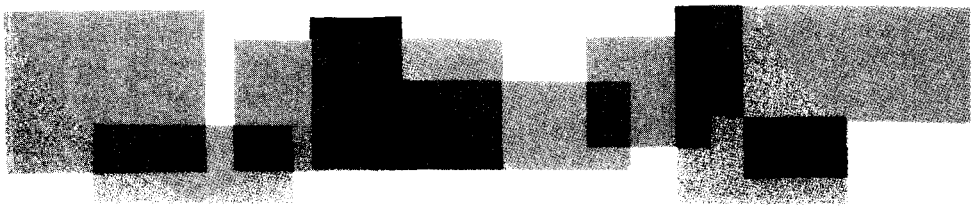
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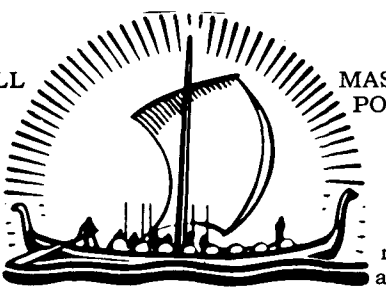
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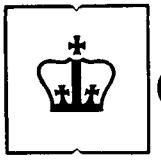
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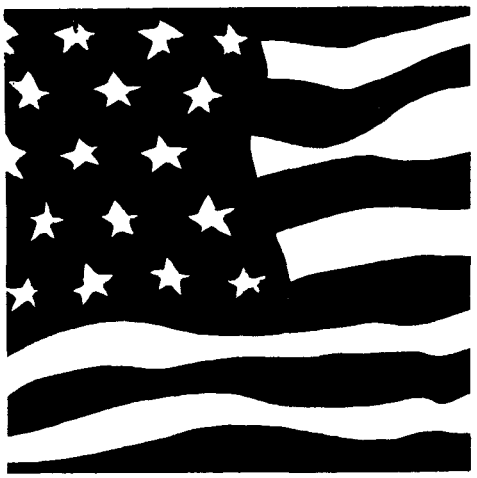
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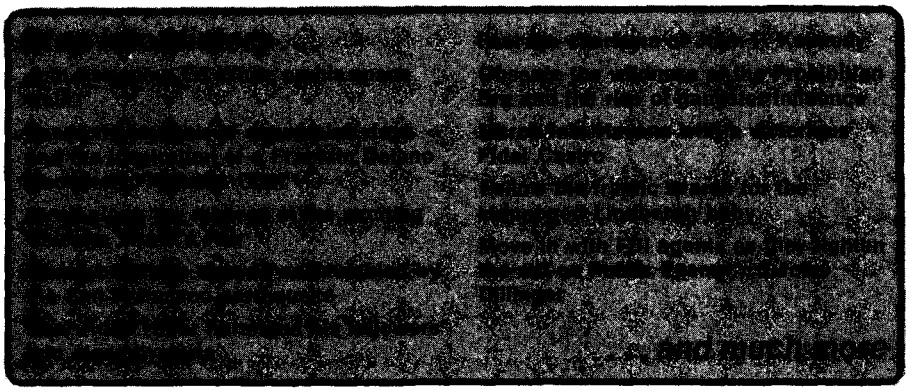
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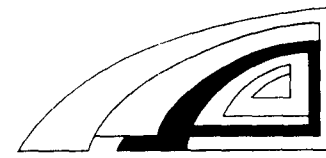
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# INDEX OF ADVERTISERS


| Advertiser | Pages |
|---|---|
| Addison-Wesley | 31, 68 |
| American Historical Association | 76, 79 |
| Barnes & Noble Books | 58 |
| Bro-Dart | 69 |
| Cambridge University Press | 26 |
| Center for Migration Studies | 63 |
| Columbia University Press | 56 |
| Cornell University Press | 6 |
| Crown Publishers | 9 |
| Dodd, Mead and Co. | 66 |
| Dorsey Press | 65 |
| Doubleday & Company | 45 |
| Facts on File | 33 |
| Florida State University Press | 73 |
| Free Press | 50, 51 |
| Harper & Row | 59, 70, 71, 72 |
| Harvard University Press | 3rd Cover |
| D. C. Heath and Company | 17, 18, 19 |
| Hill & Wang | 39 |
| History of Childhood Quarterly | 43 |
| Hoover Institution Press | 48, 49 |
| Houghton Mifflin | 29 |
| Institute for Scientific Information | 41 |
| Interland Publishing Company | 3 |
| Johns Hopkins University Press | 55 |
| Johnson Reprint | 34, 35 |
| Library Resources | 44 |
| J. B. Lippincott | 52, 53 |
| Little, Brown and Company | 46, 47 |
| Microfilming Corp. of America | 5, 37 |
| Ralph Myles Publisher | 36 |
| New York University Press | 4 |
| Ohio State University Press | 74 |
| Oxford University Press | 2nd Cover, 21, 22, 23, 24, 25 |
| Pantheon Books | 27 |
| F. E. Peacock | 38 |
| Praeger Publishers | 10 |
| Princeton University Press | 4th Cover |
| Rand McNally | 12, 13 |
| Random House | 11 |
| Redgrave Information Resources Corp. | 40 |
| St. Martin's Press | 7, 42 |
| Saturday Review Press | 67 |
| Shocken Books | 57 |
| Charles Scribner's Sons | 60, 61, 62 |
| Stanford University Press | 30 |
| Syracuse University Press | 14 |
| Teachers Insurance and Annuity Assn. | 20 |
| Temple University | 16 |
| University of California Press | 8 |
| University of Chicago Press | 15 |
| University of North Carolina Press | 28 |
| University of Wisconsin Press | 64 |
| Viking Press | 54 |
| Vintage Books | 75 |
| Yale University Press | 32 |


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